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**An Empirical Analysis of the  
Intergenerational Effects of Education  
and Policy Interventions Targeted at  
Socio-economically Disadvantaged  
Students**

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**A thesis submitted in fulfilment of the requirements for  
the degree of Doctor of Philosophy**

**University of Warwick**

**Department of Economics**

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## **Table of Contents:**

<b>List of Figures.....</b>	<b>4</b>
<b>Figures in Chapter One .....</b>	<b>4</b>
<b>Figures in Chapter Two.....</b>	<b>4</b>
<b>Figures in Chapter Three .....</b>	<b>4</b>
<b>Tables in Chapter One.....</b>	<b>6</b>
<b>Tables in Chapter Two .....</b>	<b>6</b>
<b>Tables in Chapter Three .....</b>	<b>7</b>
<b>Declaration.....</b>	<b>8</b>
<b>Acknowledgements .....</b>	<b>9</b>
<b>Dedication .....</b>	<b>11</b>
<b>Overview .....</b>	<b>14</b>
<b>Chapter One: The Impact of Parental Income and Education on the Schooling of Children .....</b>	<b>16</b>
<b>1.1 Introduction.....</b>	<b>17</b>
<b>1.2 Previous Literature .....</b>	<b>20</b>
<b>1.3 Data, Sample Selection and Sources of Exogenous Variation .....</b>	<b>28</b>
<b>1.4 Estimates .....</b>	<b>36</b>
<b>1.5 Conclusion .....</b>	<b>46</b>
<b>References for Chapter One .....</b>	<b>48</b>

<b>Figures for Chapter One .....</b>	<b>54</b>
<b>Tables for Chapter One.....</b>	<b>57</b>
<b>Appendix for Chapter One .....</b>	<b>65</b>
<b>Chapter Two: Money, Mentoring and Making Friends: The Impact of a Multidimensional Access Programme on Student Performance.....</b>	<b>73</b>
<b>2.1 Introduction.....</b>	<b>74</b>
<b>2.2 Description of the Access Program .....</b>	<b>80</b>
<b>2.3 Estimation &amp; Data Issues .....</b>	<b>85</b>
<b>2.4 Results .....</b>	<b>98</b>
<b>2.5 Conclusion .....</b>	<b>106</b>
<b>References for Chapter Two .....</b>	<b>111</b>
<b>Figures for Chapter Two.....</b>	<b>117</b>
<b>Tables for Chapter Two .....</b>	<b>122</b>
<b>Appendix for Chapter Two.....</b>	<b>136</b>
<b>Chapter Three: Piloting the Nation. What Do We Learn From Pilot Interventions? ..</b>	<b>137</b>
<b>3.1 Introduction.....</b>	<b>138</b>
<b>3.2 Policy Context.....</b>	<b>142</b>
<b>3.3 Literature Review .....</b>	<b>145</b>
<b>3.5 Results .....</b>	<b>157</b>
<b>3.5.1 Effect of the Pilot Scheme.....</b>	<b>157</b>

3.5.2	Implementation of National Scheme .....	159
3.6	Discussion of Results.....	161
3.7	Conclusion .....	166
	References for Chapter Three .....	168
	Figures for Chapter Three .....	171
	Tables for Chapter Three.....	176
	Appendix for Chapter Three .....	188

## **List of Figures**

### **Figures in Chapter One**

Figure 1.1 Post Compulsory Participation by Paternal Education.....	54
Figure 1.2 Post Compulsory Participation by Maternal Education .....	54
Figure 1.3 Distribution of Paternal School Leaving Age by Third of Year of Birth.....	55
Figure 1.4 Distribution of Maternal School Leaving Age by Third of Year of Birth.....	55
Figure 1.5 Distribution of Father's Weekly Earnings by Union Status.....	56
Figure 1.6 Average School Leaving Age by Year of Birth: England Only .....	56

### **Figures in Chapter Two**

Figure 2.1 Program Expansion by Geographical Location in Ireland .....	118
Figure 2.2 Program Expansion by Geographical Location in Dublin .....	119
Figure 2.3 University Faculty by Treatment and Control Groups Before and After the 2001 Reform .....	120
Figure 2.4 Distribution of High School Grades in Final State Exam by Treatment and Control Groups.....	121

### **Figures in Chapter Three**

Figure 3.1 Participation in Post-Compulsory Education by Age: England 1985-2007.....	171
Figure 3.2 Participation in Post-Compulsory Education .....	172
Figure 3.3 Rate of Unemployment in UK for 16-17 year olds .....	173
Figure 3.4 Distribution of Household Income and EMA Eligibility Criteria .....	174

Figure 3.5 Participation in Post-Compulsory Education .....	175
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## **List of Tables**

### **Tables in Chapter One**

Table 1.1 Sample Selection.....	57
Table 1.2 Descriptive Statistics LFS 1992-2006 Estimation Sample .....	58
Table 1.3 Effects of Parental Education and Income on the Probability of Post-Compulsory Schooling of Children .....	59
Table 1.4 First Stage Regressions .....	61
Table 1.5 Instrumental Variable Estimates: LFS 1992-2006.....	63

### **Tables in Chapter Two**

Table 2.1 Program Expansion and Commuting Distance from High Schools to University	122
Table 2.2 Labour Market Characteristics in Access Program School Localities by Year of Linkage .....	123
Table 2.3 Average High School Grades in Final State Exam for Treatment and Control Groups by Year of Linkage.....	124
Table 2.4 Student Characteristics .....	125
Table 2.5 First and Final Year Student Outcome Variables .....	127
Table 2.6 Base Results: Impact of the Access Program on First and Final Year Outcomes .	128
Table 2.7 Robustness and Sensitivity Results: Impact of the Access Program on First and Final Year Outcomes .....	130
Table 2.8 Impact of Variation in Financial Aid on First Year Outcomes.....	133



Table 2.9 Impact of Access Program on Student Performance for Students from Limited and Full Pre-entry Support Schools .....	134
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### **Tables in Chapter Three**

Table 3.1 Trends in Post-Compulsory Education Pre-Treatment. LEA Level Data.....	176
Table 3.2 Descriptive Statistics of the Family Backgrounds and Characteristics of YCS Respondents .....	177
Table 3.3 Estimates of Eligibility for EMA.....	179
Table 3.4 Estimates of Eligibility for EMA.....	180
Table 3.5 Background of YCS respondents by EMA eligibility .....	181
Table 3.6 Linear Probability Model– Effect of EMA Pilot Scheme on Post-Compulsory Full-Time Education of Eligible Youth .....	183
Table 3.7 Linear Probability Model– Effect of EMA Roll-Out on Post-Compulsory Full-Time Education of Eligible Youth.....	184
Table 3.8 Linear Probability Model – Effect of EMA Pilot and Roll Out on Post-Compulsory Full Time Education by GCSE Achievement .....	185
Table 3.9 Rates of Minimum Wage & Average Youth Wages .....	186
Table 3.10 Linear Probability Model – Effect of EMA Pilot Scheme on Post-Compulsory Full-Time Education of Eligible Youth in Areas Where More Than 10% of Students Receive Free School Meals.....	187

## **Declaration**

This thesis is submitted to the University of Warwick in accordance with the requirements of the degree of Doctor of Philosophy. I declare that any material contained in this thesis has not been submitted for a degree to any other university.

I declare that Chapter One: “The Impact of Parental Income and Education on the Schooling of Children” is co-authored with Arnaud Chevalier (Royal Holloway, University of London, Geary Institute, University College Dublin and IZA), Colm Harmon (Geary Institute, University College Dublin, CEPR and IZA) and Ian Walker (Lancaster University Management School and IZA). A much earlier version of this chapter appeared as IZA Working Paper 1496 in 2005. However significant changes have been made to this work. The current version appears in the Geary Institute Working Paper Series as W.P. No. 201032.

Chapter Two: “Money, Mentoring and Making Friends: The Impact of a Multidimensional Access Programme on Student Performance” is co-authored with Orla Doyle, Kevin Denny, Patricia O’ Reilly (all Geary Institute, University College Dublin). This chapter appears as a working paper in The Warwick Economics Research Paper Series as W.P. No. 932 and in the Geary Institute Working Paper Series as W.P. No. 201021

Chapter Three: “Piloting the Nation. What do We Learn from Pilot Interventions?” is co-authored with Arnaud Chevalier (Royal Holloway, University of London, Geary Institute University College Dublin, and IZA).

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## Overview

The over-arching theme of this thesis is the effects of parental background on children and the effectiveness of policies designed to improve the academic outcomes of socio-economically disadvantaged students. The first chapter of this thesis explores the causal link between the education of one generation and that of their children by using IV to account for the endogeneity of parental education and paternal earnings. The second chapter evaluates the effectiveness of an intervention designed to improve the academic success at university of students from socio-economically disadvantaged families. The third and final chapter examines the potential issues in expanding a programme targeted at financially poorer students beyond its initial pilot phase.

Chapter One addresses the intergenerational transmission of education and investigates the extent to which early school leaving (at age 16) may be due to variations in parental background. An important contribution of the chapter is to distinguish between the causal effects of parental income and parental education levels. Least squares estimation reveals conventional results – weak effects of income (when the child is 16), stronger effects of maternal education than paternal, and stronger effects on sons than daughters. We find that the education effects remain significant even when household income is included. However, when we use instrumental variable methods to simultaneously account for the endogeneity of parental education and paternal income, only maternal education remains significant (for daughters only) and becomes stronger. These estimates are consistent to various set of instruments. The impact of paternal income varies between specifications but becomes insignificant in our preferred specification. Our results provide limited evidence that policies alleviating income constraints at age 16 can alter schooling decisions but that policies increasing permanent income would lead to increased participation (especially for daughters).



Chapter Two is an evaluation of a comprehensive university access programme that provides financial, academic and social support to low socioeconomic students using a natural experiment which exploits the time variation in the expansion of the programme across high schools. Overall, we identify positive treatment effects on retention rates, exam performance and graduation rates, with the impact often stronger for higher ability students. Gender differences are also identified. We find similar results for access students entering through the standard admissions system and those entering with lower grades. This suggests that access programmes can be effective at improving academic outcomes for socio-economically disadvantaged students.

In Chapter Three, we compare the effects of the pilot implementation and the subsequent national roll-out of a large programme, the Educational Maintenance Allowance, in the UK which provides financial transfers to youth who remain in post-compulsory education. While piloting policies is becoming standard in policy evaluation, little is known of their external validity. Using a difference-in-difference-in-differences methodology and several cohorts of the Youth Cohort Study for England and Wales, we estimate the effect of the Education Maintenance Allowance on post-compulsory school participation both in the piloting stage and in its national implementation. We find that the pilot scheme and the national extension had an effect on post-16 schooling but that the evidence in support of the national extension is weaker.

# **Chapter One: The Impact of Parental Income and Education on the Schooling of Children**

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## 1.1 Introduction

A considerable literature has focused on the effects of parental background on outcomes of their children such as cognitive skills, education, health and subsequent income (for a review see Black and Devereux, 2010). There is little doubt that economic status is positively correlated across generations. Parents affect the behaviour and decisions taken by their children through genetic transmission, environment, and preferences. The view that more educated parents can provide a “better” environment for their children has been the basis of many interventions.

While the existence of intergenerational correlations is hardly disputed, the nature of the policy interventions that are suggested depends critically on the characteristics of the intergenerational transmission mechanism and the extent to which the relationship is causal. In particular, it has proven difficult to determine whether the transmission mechanism works through inherited genetic factors or environmental factors and, to the extent that it is the latter, what is the relative importance of education and income? Moreover, the link between the schooling of parents and their children could be due to unobserved inherited characteristics rather than a causal effect of parental education or income *per se* in household production. This issue is explored in detail in the review by Bjorklund and Salvanes (2010).

The scientific literature is not entirely clear but it is widely believed that, while raising the education of both mothers and fathers has broadly similar effects on household income, the external effects on children associated with parental education are larger for maternal education than for paternal because mothers tend to be the main provider of care within the household. For example, a positive relationship between maternal education and their child's birth weight, which is a strong predictor of child health, is found not only in the developing

world but also in the US (see, for example, Currie and Moretti, 2003). The existence of such externalities provides an important argument for subsidizing education, especially in households with low income and/or low educated parents. Indeed there may be multiplier effects since policy interventions that increase educational attainment for one generation may create spillovers to later generations.

A neglected issue is to understand the mechanisms by which parental education may affect children's outcomes. That is, parental education may be a direct input into the production function that generates the quality of the endowments that children have in various domains (health, ability etc.), may affect the choice of other inputs, and may indirectly facilitate a higher quantity and/or quality of other inputs through its effect on household income. The use of policy instruments such as income transfers to attempt to break the cycle of disadvantage presumes this latter route is important. Moreover, once one controls for education (as a long-run determinant of the level of permanent income), current income is likely to pick up the effect of income shocks that would matter only in the presence of credit market constraints.

This chapter addresses an important issue in the existing literature: the causal effect of parental education on children, allowing for separate effects of maternal and paternal education; and the causal effect of household income controlling for education. To date no study has simultaneously tried to account for the endogeneity of both parental education and parental income. The distinction between education and income is important since differences in policy approaches hang on their relative effects. Using a British cross-section dataset, we

begin by confirming the usual finding using least squares - that parental education levels are positively associated with good child outcomes, in particular later school leaving.<sup>1</sup> This outcome measure is important because the UK government has targeted a reduction in the proportion of pupils leaving at 16, and committed itself to a phased increase in the minimum age at which youths can leave education and training. We go on to use instrumental variable methods to take account of the endogeneity of *both* parental income and education. We exploit a variety of ideas for identification that have been used in other research, including changes in the minimum school leaving age for the parents, month of birth of the parents which captures early school tracking that affected the parental cohorts, and parental union status and its interactions with occupation.

The plan of the paper is as follows. Section 1.2 outlines the existing literature. Section 1.3 explains the nature of the data used. Section 1.4 provides the base estimates, which are extended and subjected to robustness checks in Section 1.5. Section 1.6 concludes.

<sup>1</sup> We also investigate the relative effects of parental education levels and household income on educational achievement at age 16. High school students in England and Wales usually study up to ten subjects until the age of 16 which are then examined at the end of compulsory schooling in the school year that they reach 16. These are scored as A\* to F with A\* -C being regarded as passing grades. The government's objective is that 60% of all 16 year olds pass in at least five subjects. This level of achievement is usually required to progress into senior high school.

## 1.2 Previous Literature

It is widely thought that children brought up in less favourable conditions obtain less education despite the large financial returns to schooling (Heckman and Masterov, 2004) and indeed there is a large correlation between the education level of parents and their children (Bjorklund and Salvanes, 2010). However the transmission mechanism behind such intergenerational correlations is not clear. Krueger (2004) reviews various contributions supporting the view that financial constraints significantly impact on educational attainment. On the contrary, Carneiro and Heckman (2003) suggests that *current* parental income does not explain child educational choices, but that family fixed effects that contribute to *permanent* income, such as parental education levels, have a much more positive role. This is the central conclusion of Cameron and Heckman (1998) using US data, and Chevalier and Lanot (2002) using the UK National Child Development Study data. Chevalier (2004), using the UK Family Resources Survey cross-section data, finds that including father's income in the schooling choice equation of the child, while itself having a significant and positive effect, does not dramatically change the magnitude of the parental education coefficients. However, the potential endogeneity of income means that this correlation does not necessarily imply that parental income matters for children's human capital accumulation. Indeed if income is endogenous and is correlated with parental education levels, then the education coefficients are also biased.

In the literature to date, researchers have attempted to identify the exogenous effect of either parental education or of parental income, but not both effects simultaneously. The literature on estimating the causal effect of parental education on the child's educational attainment has relied on three identification strategies: instrumental variables, adopted children, and twins.

The first identification strategy is to use instrumental variables methods based on ‘natural’ experiments or policy reforms that change the educational distribution of the parents without directly affecting children. Black *et al.* (2003) exploit Norwegian educational reforms which raised the minimum number of years of compulsory schooling over a period of time and at differential rates between regions of the country. Some parents experienced an extra year of education compared to other parents who were similar to them in other respects except birth year. This discontinuity is exploited to identify the effect of parental education on their children’s education. They find evidence of the impact of parental education in the OLS estimates of education outcomes for the children but estimates based on IV show no such effect, with the exception of (weak) evidence of mother/son influences. However, Oreopoulos *et al.* (2006) using the same approach and pooling US Census data from 1960, 1970 and 1980 report that an increase in parental education by one year decreases the probability of a child repeating a schooling year (or grade) by between two and seven percentage points.

The UK provides similar policy changes which are exploited in Chevalier (2004) and Galindo-Rueda (2003). Changes in the minimum school leaving age which occurred just after World War II and again in the early 1970s meant that the educational choices of future parents was exogenously affected, at least for those wishing to leave school at the earliest age. Chevalier (2004) finds that for both parents, OLS estimates of the effect of one year of parental education on the probability of post-compulsory education is about 4%, with the effects slightly larger for sons than daughters. Using the 1974 change in the school leaving age legislation as an instrument for parental education, the effects of a parent’s education on the child of the same gender *increased* substantially (for a sample of biological parents).

Galindo-Rueda (2003) exploited the earlier 1947 reform and, relying on regression discontinuity, find significant causal effects - but only for fathers.

Of course, the minimum school leaving age is likely to affect the bottom of the schooling distribution more than the top so there is a clear case for thinking, in a heterogeneous effects model, that such estimates will provide only LATE estimates that are not strictly comparable to OLS. However, to the extent that policymakers are particularly concerned about early school leavers such estimates are still of interest. Other instruments, such as the 1968 rioting of French students (Maurin and McNally, 2008), or exogenous changes in the cost of education (Carneiro et al, 2007), or the GI bill (Page, 2009) all tend to support a positive causal effect of parental education on the human capital of children.

An alternative strategy to account for genetic effects is to compare adopted and natural children. Sacerdote (2007) report that, controlling for ability and assortative mating, the positive effect of maternal education on children's education remains. Plug (2004) finds that paternal education matters more than maternal (which becomes insignificant) when the two parental effects are included in the adopted sample and that income does not affect these conclusions. This literature assumes that the presence of adopted children is uncorrelated with unobservables across families. However adopted and natural children may have different characteristics, be treated differently in school or by society (especially when of different race from their parents), or may have incurred some stigma from adoption. Additionally, adoptive families may provide a different environment to their adopted children than to their biological children such as more (or less) attention to the adopted child. As evidence of differences in the environment of adopted and natural children, Maughan *et al* (1998) find that adoptees performed more positively than non-adopted children from similar families on childhood tests of reading, mathematics, and general ability. Bjorklund *et al*.



(2006) uses a register of Swedish adoptees, which allows controls for both natural and adoptive parents' education. After correcting for the potential bias caused by non-randomness in this population, they find that genetics account for about 50% of the correlation in education between generations but also that the causal effect of adoptive parents' education remains highly significant.

Finally, Behrman and Rosenzweig (2002) use the Minnesota Twins Register female twin pairs to examine education levels of their children (who are therefore cousins) to eliminate the effects of "nurture", and that part of the "nature" effect associated with the mother (together with some of the effects of father through the associative mating). Based on simple least squares models using data on just the children and their mothers, they find large effects: one year of maternal schooling increased children's years of education by 13% (approximately half a year) while the effect of paternal schooling was about twice as large. However the between-cousins estimates of maternal education effects, which therefore control for the genetic background of the cousins (at least through their mothers) are negative, albeit insignificantly so. This contradicts the general view that maternal schooling has a positive effect on the achievement of their children. In a critical analysis of the Behrman and Rosenzweig (2002) data, Antonovics and Goldberger (2004) show that the results are quite sensitive to the selection of children who have completed education and who are aged 18 and over, rather than 16 and over. However, Behrman, Rosenzweig and Zhang (2004) repeat the original analysis on a large Chinese dataset and find strong support for the earlier Minnesota analysis.

Holmlund, Lindhal and Plug (2008) investigate whether the disparities in results are due to differences in the sample used or to the identification strategies. Using Swedish registered data they can implement the three methods, i.e. twins, adoptees and IV. Their

results are consistent with the weight of the existing literature. In twin studies, the maternal effect is small and about half of the paternal education effect. This conclusion is reversed when using adoptee samples. When relying on IV to estimate the causal effect of parental education, the paternal effect is never significant but the maternal effect is quite large. They also find that there are non-linearities in the effect of education with the effect of parental education being larger at higher levels of education.

Understanding the mechanism through which parental income affects child educational outcomes can potentially greatly improve our understanding of intergenerational mobility more generally. For example, there has been much recent debate about whether intergenerational income mobility in the UK is rising or falling. Blanden, Goodman, Gregg and Machin (2004) found that income mobility had fallen between the 1958 NCDS and the 1970 BCS. However Goldthorpe and Jackson (2007) find that intergenerational mobility, in terms of social class rather than income, has not declined when using the same data. However Blanden, Gregg and MacMillan (2008) reconcile the ostensibly contradictory results of Blanden and Gregg (2005) and Goldthorpe and Jackson (2007) by showing that the differences in intergenerational mobility of income are largely within social classes. Knowing more about the causal mechanisms driving intergenerational transfers would greatly inform these debates.

The literature on the causal effects of parental *earnings* or *incomes* on educational outcomes is not as extensive as the literature on parental education. Random assignment experiments are potentially informative but uncommon. Blanden and Gregg (2004) review US and UK evidence on the effects of policy changes which largely focus on improving short-term family finances (see also Almond and Currie, 2010). These include initiatives such as the Moving to Opportunity (MTO) experiments in the US, which provide financial

support associated with higher housing costs from moving to more affluent areas. MTO programmes are associated with noticeable improvements in child behaviour and test scores, but whether these are caused by the financial gain, changes in the physical environment, school effects, and/or peer-group changes remains unclear<sup>2</sup>. Other US work uses welfare-to-work reforms but again the income changes are accompanied by other behavioural changes – for example such reforms are aimed at increasing parental labour supply, which may also affect child attainment<sup>3</sup>.

Sibling-based studies exploit differential outcomes and incomes but it is far from clear that parents do not take compensatory actions in the face of differential financial resources associated with each sibling. If they do, then sibling studies estimate the effects net of those actions. Other studies look at value added in the form of changes in outcomes associated with variation in income over time to difference out unobserved heterogeneity. Similar studies use early measures of outcomes as controls for unobserved heterogeneity. However, estimation of such lagged dependent variable models are, in general, inconsistent in the presence of fixed child or family effects. Nor are they really very satisfactory ways of dealing with endogeneity because income may, itself, respond to lagged outcomes – for example, a failing child may stimulate a parent to work harder, to provide more financial resources to allow the child to improve.

<sup>2</sup> Work on MTO by Sanbonmatsu *et al* (2004) suggests that MTO-driven neighbourhood effects on academic achievement were not significant.

<sup>3</sup> In the UK, the pilots of Educational Maintenance Allowances (EMA's) provided a sizeable means tested cash benefit conditional on participation in education and paid, depending on pilot scheme, either to the parents or directly to the child (see Department for Education and Skills, 2002). Enrollments increased by up to 6% in families eligible for full subsidies. However, this transfer was conditional on staying in school and so this reform is not directly informative about the effects of unconditional variations in income.

In the absence of convincing experimental evidence, and because of doubts over the validity of sibling-based studies, instrumental variables have been used to identify the effect of parental income effects on child outcomes. Shea (2000) uses union status (and occupation) as an instrument for parental income. The identifying assumption is that unionized fathers are not more ‘able’ parents than nonunionized fathers with similar observable skills. Meyer (1997) uses variation in family income caused by state welfare rules, income sources, and income before and after the education period of the child, as well as changes in income inequality. While strong identification assumptions are used in both these studies, they both find that unanticipated changes in parental long-run income have only modest and sometimes negligible effects on the human capital of the children<sup>4</sup>.

Blanden and Gregg (2004), using UK data, find the correlation between family income and children’s educational attainment has actually risen between the British Cohort Study of children born in a particular week in 1970 and the later British Household Panel Survey data which contains children reaching 16 through the 1990’s. They estimate the causal effect of family income in ordered probit models of child’s educational attainment (from no qualification up to degree level) based on sibling differences in the panel data. They also provide estimates of the probability of staying-on at school past the minimum age of 16. However the paper cannot simultaneously provide estimates of the causal effect of

<sup>4</sup> Acemoglu and Pishke (2001) use similar arguments to Meyer (1997) and exploit changes in the family income distribution between the 1970’s and 1990’s. They find a 10 percent increase in family income is associated with a 1.4% increase in the probability of attending a four-year college. Loken (2010) studies the long-term effect of family income on children’s educational attainment using the Norwegian oil shock in the 1970s as an instrument. They find no causal relationship.

parental education because this is treated as a fixed effect in the sibling difference estimates and thus differenced out.

Finally, Jenkins and Schluter (2002) is notable for being one of the few studies to control both for income, at various ages, and education. They study the type of school attended (vocational or academic), using a small German dataset, they find that later income is more important than early income, but that income effects are small relative to education effects. The analysis in their paper, as in Blanden and Gregg (2004), assumes the exogeneity of income and parental education.

### 1.3 Data, Sample Selection and Sources of Exogenous Variation

Research on this topic requires data on two family generations in a single data source – the education of the children and the education and incomes of their parents. Our analysis is based on the Labour Force Survey (LFS) - a quarterly survey of households in the U.K. In each quarter there are roughly 120,000-160,000 respondents (more in earlier quarters) from the approximately 50,000-65,000 households surveyed. Households are surveyed for five consecutive quarters. We pool the data from households in the fifth quarter over the period 1993-2006<sup>5</sup>. Children aged 16 to 18 living at home are interviewed in the LFS, and so parental information can be matched to the child's record<sup>6</sup>. Our sub-sample consists of those children observed in LFS at ages 16 to 18 inclusive (and therefore have made their decision with respect to post compulsory education participation) which is approximately 43,000 observations, or 4% of all LFS respondents (which corresponds closely to the population census data).

The key outcome of interest in this chapter is the decision to participate in post-compulsory schooling, defined as a dummy equal to one if the 16 to 18 year old child is either in post compulsory education at present or was in education between 16 and 18 but has left school at the time of interview (based on the age left full time education information in LFS). Note that only 16 year olds who are surveyed between September and December are

<sup>5</sup> Pre-1998, earnings data is available only for fifth wave respondents; from 1998 the earnings data is collected in the first and in the final wave. Prior to 1993 there was no earnings data in LFS. From 2006 one of our instrumental variables ceases to be available in the data.

<sup>6</sup> Chevalier (2004) uses the Family Resource Survey data that, in many respects, is similar to the LFS data in this chapter. Crucially, the LFS has information on union status which is potentially important for the identification strategy adopted in this chapter.

included to ensure information on their decision to leave or remain in education is available. The age range is limited because we need to observe respondents while they are still living at home in order to observe parental background (respondents are not asked directly about their parents). An examination of BHPS data suggests that only 6% of children aged 16-18 have already left home. However, this censoring in the LFS data becomes more severe with older teenagers - whilst 98% of 16-year-old children are observed living with both parents, this proportion is down to 88% for those 18 years old.<sup>7</sup> We also drop observations from Scotland and Northern Ireland. Although these regions changed their minimum school leaving ages at different times to England and Wales they also have quite distinct education systems<sup>8</sup>. The details of the original LFS data and the impact of the selection criteria can be seen in Table 1.1. We select teenagers where two parents are present<sup>9</sup>, and where the father is working and reporting his income, where both parents were born after 1933 (and so were not affected by the earlier raising of the school leaving from 14 to 15, and whose school leaving is unlikely to have been directly affected by World War II), and where both parents were born in the United Kingdom, and are currently resident in England or Wales. We make these restrictions in order to avoid including potentially endogenous factors that affect educational outcomes.

<sup>7</sup> Re-estimating without the 18 year olds showed no economically or statistically significant differences in results.

<sup>8</sup> The data records only region of current residence, not where the parents were educated. However, this is unimportant in our IV context. Re-estimating including observations in NI and Scotland leads to a drop in precision but no change in the magnitudes of the key parameters.

<sup>9</sup> Whilst this may create some selection bias it would be difficult to overcome this in our data. Since parental separation is probably more likely for children with large (but unobservable) propensities to leave school early, and it is also likely to be negatively correlated with parental education and income we might expect to underestimate the effect of income and education on the dependent variables.

Thus, our estimates need to be viewed as condition on these selections<sup>10</sup>. We also drop any observations where there is missing data on the variables of interest and we trim the bottom 1% and top 5% of the paternal earnings distribution.

Currently only two parent families with the father working as an employee (mother may or may not be working) are included in the analysis. We do not claim that our results can be extrapolated to other groups such as single parent families or where the child has already, by age 16-18, moved away from their parents.

If we were to include all of these other groups then controls for these heterogeneous family groups would need to be included in the second stage equation (i.e. the child outcome equation). However it is likely that such variables would be endogenous and instruments would need to be found for each. For example, single mothers may be unobservably different from married mothers and such differences may be positively or negatively correlated with child outcomes. Furthermore, including single mothers would also require modelling female labour supply which would require further instruments.

Figures 1 and 2 show the participation rate in post-compulsory schooling in our final sample broken down by paternal and maternal education. The education of the children appears closely correlated with the education of their parents, particularly up to a leaving age of 18; having parents with more education than this level does not substantially affect the staying-on probability of children which is then almost 100%<sup>11</sup>. There are some sizable gaps

<sup>10</sup> In fact, the estimated coefficients of interest were not greatly affected by these selections.

<sup>11</sup> Note that there are only few parents with school leaving age of 17, 19, 20 and above 23.



between the participation of girls and boys from lower educated parents but these gaps narrow with parental education.

Table 1.2 shows some selected statistics for the sample used in our analysis. The post-compulsory schooling participation rate is 73% for boys and 83% for girls<sup>12</sup>. There are large differences in the parental education and household income levels between those that remain in school compared to those that leave: more than one year extra parental education on average, and more than 20% higher paternal earnings.

Parental income is potentially endogenous either because it is correlated with unobservable characteristics which are correlated with the child's educational attainment, or because the parental education effect is transmitted through income. Shea (2000) estimates the impact of parental income using variation in income associated with union, industry, and job loss and finds a negligible impact on children's human capital for most families (although parental income did seem to matter for families whose father has low education). We assume that union membership status creates an exogenous change in income, which is independent of parenting ability and the child's educational choice. Indeed the raw data, presented in Table 1.2, showed that children who stay on are just as likely to have unionized fathers as children who do not stay on in education. We also exploit paternal occupation but mostly to control for differences in unionization rate by occupation. Later, the estimates that we highlight are those that rely only on paternal occupation-union interactions as exclusion

<sup>12</sup> Official statistics from the Department for Children, Schools and Families show 67% of boys and 75% of girls in the relevant cohort choosing to stay so our own staying-on figures from LFS are a little higher reflecting the selections that we have made.

restrictions, (although we find a similar pattern of results when we also use union status alone as the exclusion restriction).

Lewis (1986), and much subsequent work, demonstrates that wages vary substantially with union status, controlling for observable skills. Figure 1.5 shows the kernel densities of the earnings of union member fathers and non-union fathers. The union/non-union earnings gap for fathers in our selected sample from the raw data is 8%. If union wage premia reflect rents rather than unobserved ability differences it seems plausible to make the (stronger) identifying assumption, used in this chapter; that union status, controlling for occupation, is uncorrelated directly with the parental influence on educational outcomes of the children. Support for the view that unionization picks up differences in *labour market* productivity is mixed. Murphy and Topel (1990) find that individuals who switch union status experience wage changes that are small relative to the corresponding cross-section wage differences, suggesting that union premia are primarily due to differences in unobserved ability. However Freeman (1994) counters this view, arguing that union switches in panel data are largely spurious so that measurement error biases the union coefficient towards zero in the panel. In any event, we are assuming, as in Shea (2002), that unionized fathers (and their spouses) are not more ‘productive’ as parents than non-union fathers with similar observable skills and we

have some evidence to suggest that parenting behaviour is not very different across union status of fathers.<sup>13</sup>

Parental education is also likely to be endogenous. Here we rely on two sources of variation. Reforms to the minimum school leaving age have frequently been used as a source of exogenous variation – either exploiting natural experiments where different areas of a country changed their rules at different times, or using a national reform as a regression discontinuity by controlling for the smooth trends in school leaving, or considering just a narrow window of birth cohorts around the reform.<sup>14</sup> In this chapter we identify the effect of parental education on children's education using the exogenous variation in schooling caused by the raising of the minimum school leaving age (abbreviated as RoSLA: Raising of the School Leaving Age). Individuals born before September 1957 could leave school at 15 while those born after this date had to stay for an extra year of schooling. This policy change creates a discontinuity in the years of education attained by the parents. Figures 1.3 and 1.4 illustrate this by showing mean years of schooling by birth cohort (in 4 month periods) around the reform date. That is, we take a narrow window of birth cohorts around the reform

<sup>13</sup> The British Cohort Study (BCS) data, of all children born in England and Wales in a particular week in 1970, records, in considerable detail, the attitudes and behaviours of fathers towards their children. This data suggests small differences in attitudes and behaviours across union status. For example, 23% of unionised fathers disagreed with the statement that "The needs of children are more important than one's own", compared to 18% of the non-unionised; 60% (62%) of children with unionised (non-unionised) fathers watched TV less than 2 hours per day on a typical weekend day; 83% (88%) of unionised (non-unionised) fathers read stories more than once per week 57% (52%) of unionised (non-unionised) fathers always (as opposed to often/sometimes/never) talked to his child even when busy; 79% (79%) of unionised (non-unionised) fathers showed the child physical affection at least once per day and 36% (37%) praised the child at least once per day; 94% (95%) of unionised (non-unionised) fathers has helped young children learn numbers, etc; and 79% (80%) of unionised (non-unionised) fathers aspired for the child to continue in full-time education at age 16. The children also reported behaviour that might well reflect parenting styles. For example, 56% (54%) of the children of unionised (non-unionised) fathers made their own bed and 49% (52%) cleaned their own room.

<sup>14</sup> See, for example, Harmon and Walker (1995) for the UK; Black, Devereux and Salvanes (2005) for Norway; and Oreopoulos, Page and Stevens, (2006) for the USA.

(+/- four years) to minimize the influence of any long-term trends across birth cohorts. There is a marked jump in the graph for parents born after September 1957 which coincides with the introduction of the new higher school leaving age. Note that between 30% and 40% of parents left school before the new minimum, so that the reform is biting and changes the behaviour of a substantial fraction of individuals in the affected cohorts. Individuals affected by the new school leaving age have on average completed half a year more schooling than those born just before the reform. Chevalier *et al.* (2004) show that the effect of this reform was almost entirely confined to the probability of leaving at 15 relative to 16 – there is little effect higher up the years of education distribution. Hence, this reform only identifies a LATE for individuals with low levels of education. Table 1.2 shows that the proportion of fathers who were born before the RoSLA reform is higher than for mothers, reflecting their slightly greater age, and the table also shows that early leavers typically have slightly younger parents.

A second source of variation in parental schooling that we exploit derives from parental month of birth (exploited by Crawford *et al.* (2007)). There are several ways in which month of birth can affect the parents' education levels: through entry timing, whole group teaching, developmental differences, and through peer effects. The academic year starts in September but the traditional admissions policy that reigned in the 1950's and 1960's, when most of the parents in our data were young, allowed entry at the start of the term that the child turns 5 so that there were three points of entry each year: September, January and April/May. Thus the August born would start in April/May and have two fewer terms in primary school than their classmates. A school cohort would consist of children born within a 12-month window. In the 1950's and 60's whole class teaching was the dominant teaching method and development differences might imply that the youngest and the oldest

might fare worse than the average. Peer effects might arise because the youngest might be dominated or intimidated by the oldest. The year group moves as a single unit through the academic system so that they sit examinations at same time and would be at different development ages when facing the same examination.

Moreover, most of the parents in the data would have faced a selective schooling system where children were segregated into academic or vocational schools at the age of 11 based on a single test conducted on the same day across the whole country - known as the 11+ exam. Based on the results of this test, children were educated either in vocational or academic tracks. Children in the vocational track were more likely to leave school at the minimum compulsory age, while those for the latter could go on to higher secondary school and university (see Harmon and Walker, 1995). These two different types of schools placed quite different expectations on the children and there was very little movement between school types after the age of 11. Figure 1.6 shows, by year of birth, the average age at which the parents in our data left full-time education for those who were September born, the eldest in their class cohort, compared to those who were July born, the youngest<sup>15</sup>. The typical difference in years of schooling between the September and July born was around  $\frac{1}{4}$  of a year for these cohorts born in the 50's and 60's. Notice that the gap closed completely for cohorts born in the early 1960's when the 11+ examination was abandoned in most areas. So the month of birth effect in educational achievement seems to be mostly driven by the early tracking faced by these cohorts.

<sup>15</sup> We use July rather than August for this comparison since there is likely to be some ambiguity with August-born children to the extent that schools exercised discretion at the margin.

## 1.4 Estimates

Our basic model of the impact of parental background on the post-compulsory schooling participation of their children is:

$$(1) \quad PC_c = a(S_m, S_f) + \gamma Y_h + \mathbf{X}_h' \boldsymbol{\delta} + f(DB_c, DB_m, DB_f) + \varepsilon_c$$

where the  $c$ ,  $m$  and  $f$  subscripts refer to the child, mother's and father's characteristics within a particular household  $h$ . The dependent variable  $PC_c$  is a dummy variable defining participation in post compulsory education. This is estimated as a linear probability model to subsequently facilitate the use of instrumental variables, and is a function,  $a(\cdot)$ , of parental education levels measured in years of schooling of both the mother and father ( $S_m$ ,  $S_f$ ), and log parental income  $Y_h$  measured by father's real log gross weekly earnings from employment<sup>16</sup>.  $DB$  refers to date of birth (year and month) so that  $f(\cdot)$  controls for cohort trends in paternal, maternal and child education. Three different specifications of  $\mathbf{X}$  are used. First  $\mathbf{X}_h$  contains characteristics common to all three members of the family (i.e. year and month of survey dummies as well as region of residence at time of survey as well as interactions to capture region-time time effects such as local unemployment rates which may affect staying on rates). Second, we additionally condition for paternal occupation, so that the difference in unionization between occupations does not identify the IV model. Third, we

<sup>16</sup> Note that we use paternal income because the use of household income measures requires the inclusion of female earnings, which is potentially much more heavily affected by endogenous labour supply decisions. However its exclusion may also cause a bias if female labour supply is correlated with educational outcomes for children as well as with the variable of interest in the model. We share our inability to resolve this problem with the rest of the literature. If maternal labour supply is uncorrelated with paternal income and if incomes are shared within the household then our estimate of the effect of paternal income is the same as the effect of household income. This is clearly an important problem for future research.

add union status, so that the identification in the IV model only comes from the interaction terms between union status and occupation. This then captures any differences in parenting behaviour that unionized father may have.

It is difficult to include female labour supply in the model. A priori, one might think that female labour supply would affect child outcomes (although probably more so at younger ages than at the ages of people choosing post-compulsory schooling). However one could argue that the female employment decision is endogenous when modelling child outcomes. For example, female labour supply is dependent on, among other things, reservation wages and wage offers. These may be affected by factors which remain unobserved even after controlling for maternal education. Such factors might include preferences for work over staying at home and non-cognitive character traits. These in turn may affect child outcomes directly and thus there would be an endogeneity problem which would require finding another instrument in addition to those already used in the model.

The main focus of this paper is to overcome, jointly, the endogeneity problems associated with paternal education, maternal education and paternal income. Incorporating female labour supply into the model is an important issue and one that would need future investigation, perhaps with a different dataset that would allow the researcher to observe female employment at different ages of the child and child outcomes and where possible instrumental variables (e.g. presence of younger siblings, more detailed local labour market information, etc) are available.

Table 1.3 summarizes our OLS estimates of paternal income and parental education levels, where  $a(.)$  is assumed to be linear, on the probability of post-compulsory schooling of the child<sup>17</sup>. Specification (1) only controls for parental years of schooling and suggests positive, if modest, paternal and maternal education effects on the schooling choice of both sexes. The impact of a year of maternal education is an increase in the probability of post-16 participation of about 3.3% for boys and 2.6% for girls – about one percentage point lower than reported in Chevalier (2004). The impact of paternal education is somewhat lower and the effect on boys is larger than for girls. Specification (2) examines the impact of paternal income but excludes the parental education controls. These estimates suggest sizable and significant income elasticities with the effect somewhat larger for boys (20%) than for girls (14%). Finally specification (3) includes both education and income controls. The direct effects of maternal education estimated in Specification (1) are reduced very slightly in (3), but the paternal education and income effects are reduced by a factor of approximately one third compared to (1) and (2), highlighting the correlation between paternal education and income<sup>18</sup>.

The second set of estimates (4, 5, 6) in Table 1.3 adds the paternal occupation status (7 dummies). This is potentially an endogenous variable, but since unionization rate differs by occupation, without controlling for occupation the union instrument would partially

<sup>17</sup> We control for smooth cohort trends by including a cubic function of parents and child's months/years of birth. Region controls are also included, as well as survey year dummies. Similar estimates based on probit models are also available. While multiple observations of closely spaced children in each household are possible their incidence is small (just 10% of individuals have at least one other sibling in the dataset) and any improvement in standard errors from exploiting the clustering in the data would be marginal.

<sup>18</sup> The estimated income effects here are closely comparable in magnitude to the results in Blanden and Gregg (2004). See their Tables 6 and 7 in particular.



capture occupational choice which would invalidate its use. As expected, since parental occupation can be viewed as proxies for permanent income, the estimates on education are almost identical to those obtained when controlling for paternal income. Thus, in this specification, income is best interpreted as deviation from the permanent income. As such, the income effects are reduced by about 30% for boys and 50% for girls. Note, however, that when controlling for occupation, adding paternal income only marginally reduces the effect of paternal education on the educational attainment of children. Thus indicating that the correlation between paternal education and income mostly captures the permanent component of income, rather than income shocks.

While we have tried to alleviate the concern that unionized fathers differ in their parenting behaviour, our final identification strategy relies only on the *interactions* of paternal union membership and paternal occupation as instruments for paternal income. Thus, the final set of estimates in Table 1.3 show the effects of parental education and paternal earnings when controlling for the effects (not interacted) of paternal union membership and paternal occupation dummies in specifications (7), (8) and (9). The effects of parental education are virtually unchanged compared to the estimates presented in (4) and (6); supporting the view that paternal union membership has no direct effect on the education decision of his children. For girls, the effect of paternal income also remains unchanged compared to specifications (5) and (6) while for boys it decreases by less than one percentage point. To summarise these results: the effect of parental education on the decision to remain in school past compulsory age appears to be quite small - around 3% for boys and 2% for girls, and larger for maternal education than parental education. The gap between the effect of maternal and paternal education increases when measures of, or proxies for, income are introduced since the maternal education effect remains largely unaffected while paternal

education effect drops by almost a half. Note also that the income effects are severely reduced when a measure of permanent income is controlled for.

To control for the potential endogeneity of paternal income and parental schooling we specify a set of first stage equations. We define dummy variables for *RoSLA* (born after the critical date) and parental union membership (*PUM*), and its interactions with the seven occupational categories, *Occ*, which are incorporated into our first stage model. We also impose a linear structure on the month of birth effect<sup>19</sup> by including a month of birth indicator, *MoB* (which takes the value of one for September born through to twelve for August born). Therefore, in our preferred specification, we estimate a system of first stage equations, using a seemingly unrelated specification to allow for correlations between the respective residuals:

$$\begin{aligned}
 S_m &= \phi_1 RoSLA_m + \phi_2 MoB_m + \mathbf{X}'_h \phi_3 + r(DB_m) + \mu_m \\
 (2) \quad S_p &= \theta_1 RoSLA_p + \theta_2 MoB_p + \mathbf{X}'_h \theta_3 + s(DB_p) + \nu_p \\
 Y_p &= \left( PUM_p * Occ_p \right)' \pi_1 + \mathbf{X}'_h \pi_3 + \omega_p
 \end{aligned}$$

where the functions  $r(\cdot)$ , and  $s(\cdot)$  control for smooth birth cohort trends in school leaving age so that the *RoSLA* acts as a regression discontinuity and picks up the effects of the reform.

The system of equations defined above is over-identified and we estimated a wide variety of first stages and corresponding second stage equations to examine the sensitivity of the second stage estimates to the set of exclusion restrictions used to define the instrumental variables. Our IV estimates have the property, also a feature of the OLS estimates, that the

<sup>19</sup> Greater flexibility could be sought but at the cost, of course, of potential weakness in the instruments.

addition of income to the model containing just parental education levels makes little difference to the estimates. Thus, we refrain from presenting specifications that contain just parental income or just parental education levels. Table 1.4 shows different specifications of our three first stage equations. Each block refers to a different equation – the paternal schooling equation, the maternal schooling equation, and the paternal log earnings equation. The columns show specifications that vary according to which sets of instruments are used. The three equations are estimated simultaneously and the  $F$ -statistics of the different sets of instruments are presented in the bottom panel of the table. Comparing across the schooling equations we see that the inclusion of *MoB* is significant at 5% but makes little difference to the size and significance of the *RoSLA* effects. In general, the raising of the school leaving age increased parental education by between 0.25 and 0.3 of a year, the effects being almost identical for both parents. The month of birth effect is also statistically significant and negative for both males and females. An August born child, on average, left school one sixth of a year earlier than a September born child. The two instruments identify the effects of exogenous shocks to parental education through different mechanisms - so that in models where both set of instruments are included the estimates are almost identical to the ones obtained when the instruments are included individually. So while both instruments identify a population of marginal students, these are not identical populations. The paternal earnings equation shows a significant positive union membership wage premium of 7% in columns 1, 2 and 3, which are consistent with existing UK evidence. In specifications 4, 5 and 6 the interactions of union membership and occupation show that the premium is larger for manual and less skilled occupations (the reference group being Managers and Senior Administrators). The corresponding  $F$  tests for the joint significance of all of the instruments used in each specification of the exclusion restrictions indicate whether the instruments are “weak”. The

critical values of these F tests are reported in Stock and Yogo (2005) – the rule of thumb for the just identified case and one endogenous variable is approximately 10, with larger values for more exclusion restrictions. Thus our F tests vary from indicating instruments with considerable strength to being close to weak.

Table 1.5 shows the second stage estimates<sup>20</sup>. Specifications (a) and (b) replicate the OLS estimates from Table 1.3 when controlling for occupation only or for occupation and union status, respectively. The subsequent columns report the IV estimates for different specifications of the first stage. The pattern of second stage estimates of parental education effects seems remarkably stable across the IV specifications. The effect of paternal education is always imprecisely estimated when controlling for maternal education and paternal earnings regardless of which set of instruments is used and never statistically significant. Note though that the sign of the point estimates differs for sons and daughters. The effect of maternal education on daughters is between 0.187 and 0.212 depending on the instruments used and is always significant at the 1% level, and significantly higher than the OLS estimate of 0.022. The magnitude of the effect of maternal schooling on the schooling of sons also increases greatly when instrumented and reaches 0.10 in all specifications but just failed to be statistically significant. Overall, the impact of maternal schooling on the schooling decisions of her children, and especially daughters, appear to be quite large.

The effect of paternal earnings varies with the instruments used. When Paternal Union Membership alone is used as an instrument, the effect is roughly 0.26 for sons and 0.29 for

<sup>20</sup> Table 1.5 reports only the OLS and IV estimates for the specifications labelled (6) and (9) in Table 1.3 but similar stability in the second stage results can be shown in the estimates for the equivalent of the other specifications presented in Table 1.3.

daughters. If occupation, union membership and interactions of occupation and union membership are used, the effect of paternal income becomes larger for sons (0.32) but decreases (to 0.13) for daughters. However, in our preferred specification, when only the interactions of union and occupation are used as instrument and both union and occupations are controlled for, the effect of paternal income becomes much smaller especially for daughters. Even if the point estimates are still large for sons, they are statistically insignificant at even the 10% level. Thus, paternal earnings, that we take as a measure of short term variation in the household income as we are controlling for occupation, have little impact on the schooling decisions of children.

### **Measurement Error**

In our specifications we control for paternal employee earnings as reported at the time the young person is 16, 17 or 18. Blanden, Gregg and MacMillan (2008) discuss the problem of measurement error in relation to income. Like all non-systematic measurement errors, this will bias the coefficient downwards if the variable is included as a regressor. They have concerns that income is more likely to be mis-measured for those with less attachment to the labour market. These families might be relying on non-labour market sources of income which may not be well measured. In our study we are not looking at income in general but at paternal employee earnings specifically. We are not claiming that our results can be extrapolated to families relying on paternal self-employment income or solely on welfare or another source of income.

Blanden, Gregg and MacMillan (2008) also discuss the problem of measurement error in relation to the distinction between current and transitory income. They recognise that

income over the child's entire life, especially at the early stages (see Carnerio and Heckman (2003)), would have a greater influence on child outcomes at 16 than income recorded at 16. Using a variety of data sources, they argue that father's social class is not as good as a predictor of permanent income as education, housing tenure type and receipt of free school meals.

In the LFS data used in this study, it is not possible to construct a measure of permanent income as LFS is a rotating panel with addresses staying in the panel for only five quarters. However our main specifications control for both parental education and paternal social class which will proxy the effect of permanent income. Indeed the effect of paternal employment income falls significantly when paternal social class is included in the model. Thus our measure of current income could be seen as transitory deviations from lifecycle profiles of income.

### **Grade Attainment:**

The models were also estimated using whether the child attains +5 GCSE A\*-C grades as the outcome of interest. Attaining +5 GCSE A\*-C grades is often a requirement for admission to a sixth form to take A levels. The results are shown in Tables 1A1, 1A2, 1A3 & 1A4 in the appendix. Both the OLS and IV estimates are shown. The OLS estimates are very similar to those where the outcome is whether or not the child stays in full time education beyond the age of 16. In the IV estimates, we find that the effect of income on grade attainment is the same or smaller as the effect of income on staying on in post-compulsory schooling. However this varies by the instruments used. In the case where only the interactions of paternal union status and paternal socioeconomic group are used as instruments for income,

no significant effect of income is found. In the IV results for girls, no effect of maternal education on grade attainment is found.

As fewer respondents answered the questions relating to grade attainment than did so in relation to staying on in post compulsory education, the sample sizes are smaller by about 15%. To check if sample selection could explain the differences in the pattern of results between the two outcomes, the “staying on” model was estimated using the sample who had answered the grade attainment question. The overall pattern of results using this smaller sample are similar but the effect of income is less precisely estimated. These results are included in the appendix.

## 1.5 Conclusion

This chapter has addressed the intergenerational transmission of education and investigated the extent to which early school leaving (at age 16) may be due to variations in permanent income and parental education levels. Least squares revealed conventional results - stronger effects of maternal than paternal education, and stronger effects on sons than daughters. We also found that the education effects remained significant even when household income was included. Income remains significant even when some measures of permanent income are included which indicates that some children could be financially constrained in their decision to attend post-compulsory education.

When controlling for paternal income, the IV results reinforce the role of maternal education, especially for daughters, where the estimates increase almost ten fold. One year of maternal education, for mothers affected by our instruments, increases the probability of her daughter staying on by 20 percentage points. The effects on sons are only half of this and just on the border of statistical significance. In contrast, paternal education has no statistical effect on the probability of remaining in education for either son or daughter.

Accounting for the endogeneity of paternal income also increases the elasticity of income on schooling decisions, however depending on the set of instruments the effects is imprecisely estimated, and in our preferred specification becomes insignificant, even if a large point estimate is still found for sons. The income effects are in general larger for boys than for girls, this is also the case for the UK's Education Maintenance Allowance (EMA).

The results imply that policy options that are explicitly aimed at relieving credit constraints at the minimum school leaving age such as EMA (see Dearden *et al.*, 2009) may not be so effective in promoting higher levels of education (a finding that is consistent with



recent UK evidence that used linked administrative data for a cohort from age 11 to age 19 – see Chowdry *et al*, 2010). A policy of increasing permanent income, like increasing parental education (or say through Child Benefit) would on the other hand have some positive effects, especially for daughters. The recently proposed increase of the school leaving age to 18 would also benefit *future* generations through direct intergenerational transmission of educational choice.

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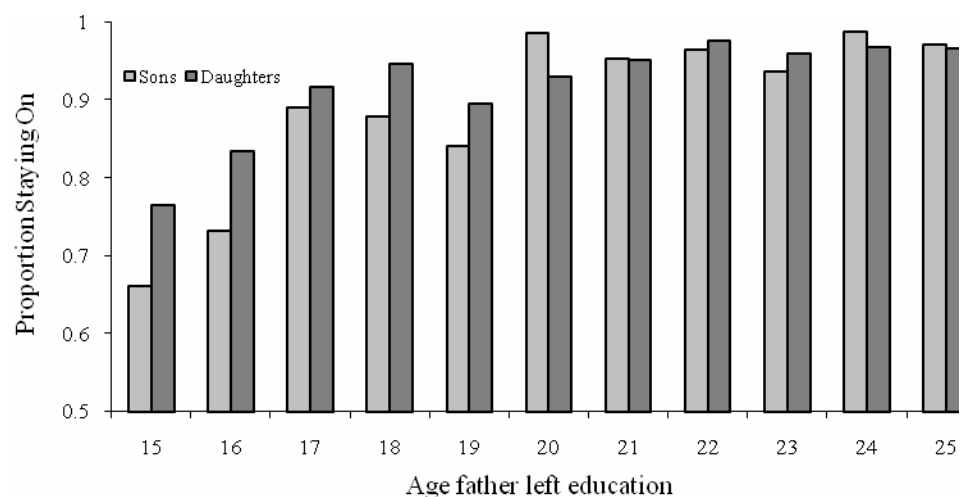
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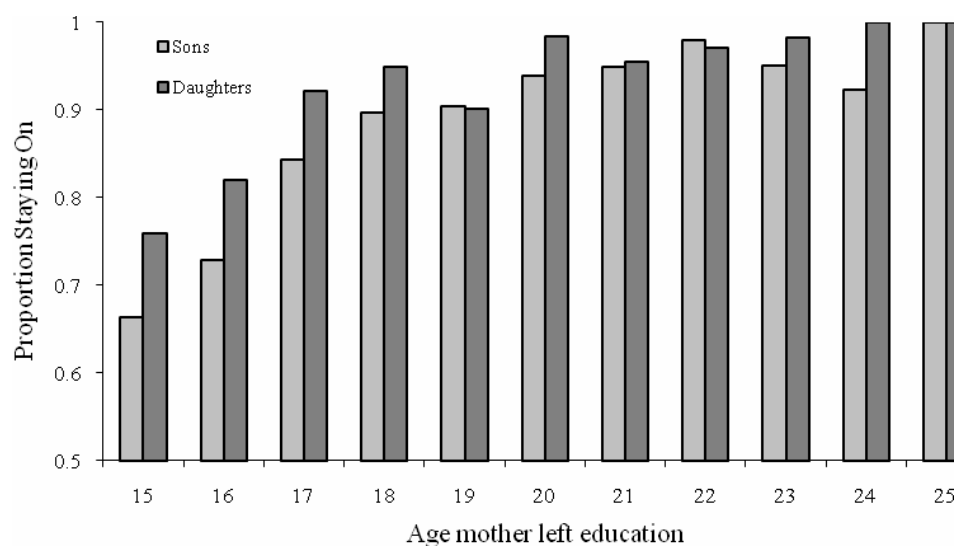
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## Figures for Chapter One

**Figure 1. 1 Post Compulsory Participation by Paternal Education**

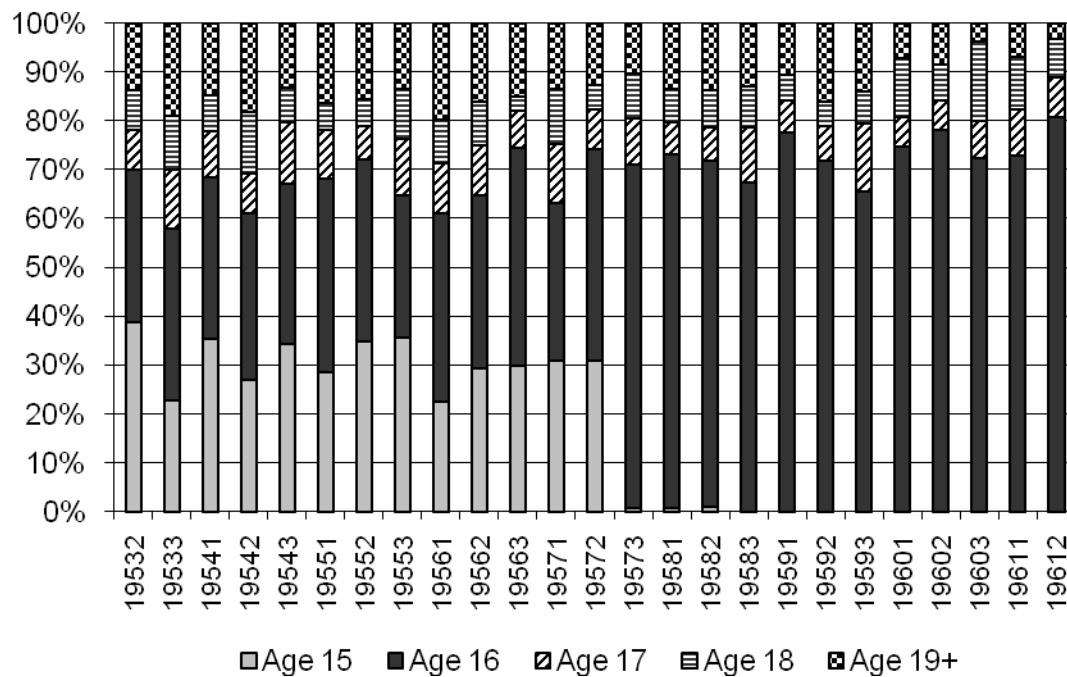


**Figure 1. 2 Post Compulsory Participation by Maternal Education**





**Figure 1. 3 Distribution of Paternal School Leaving Age by Third of Year of Birth**



**Figure 1. 4 Distribution of Maternal School Leaving Age by Third of Year of Birth**

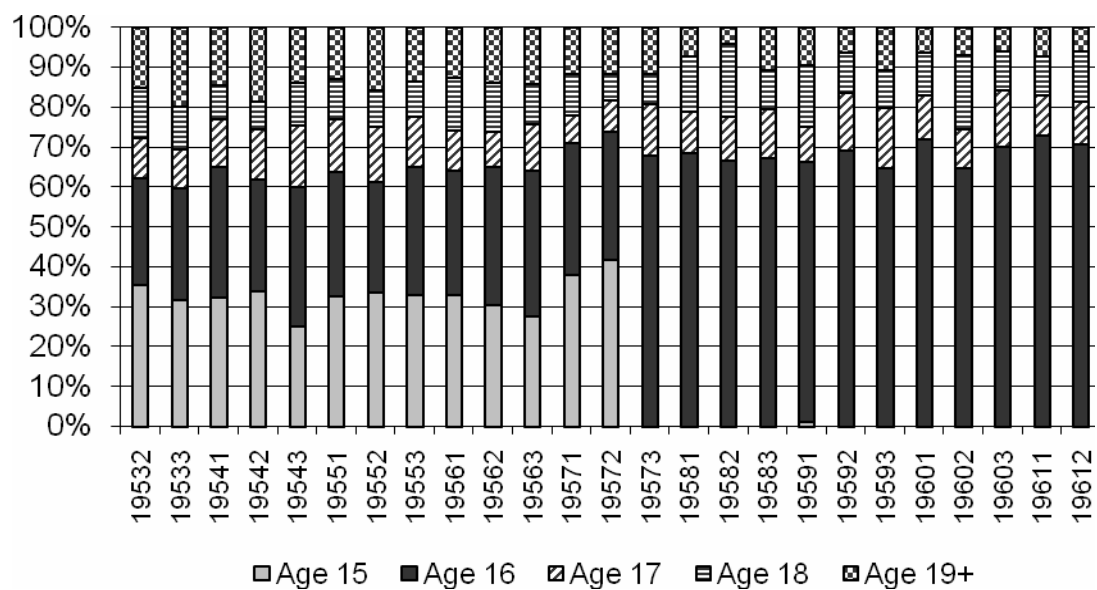


Figure 1. 5 Distribution of Father’s Weekly Earnings by Union Status

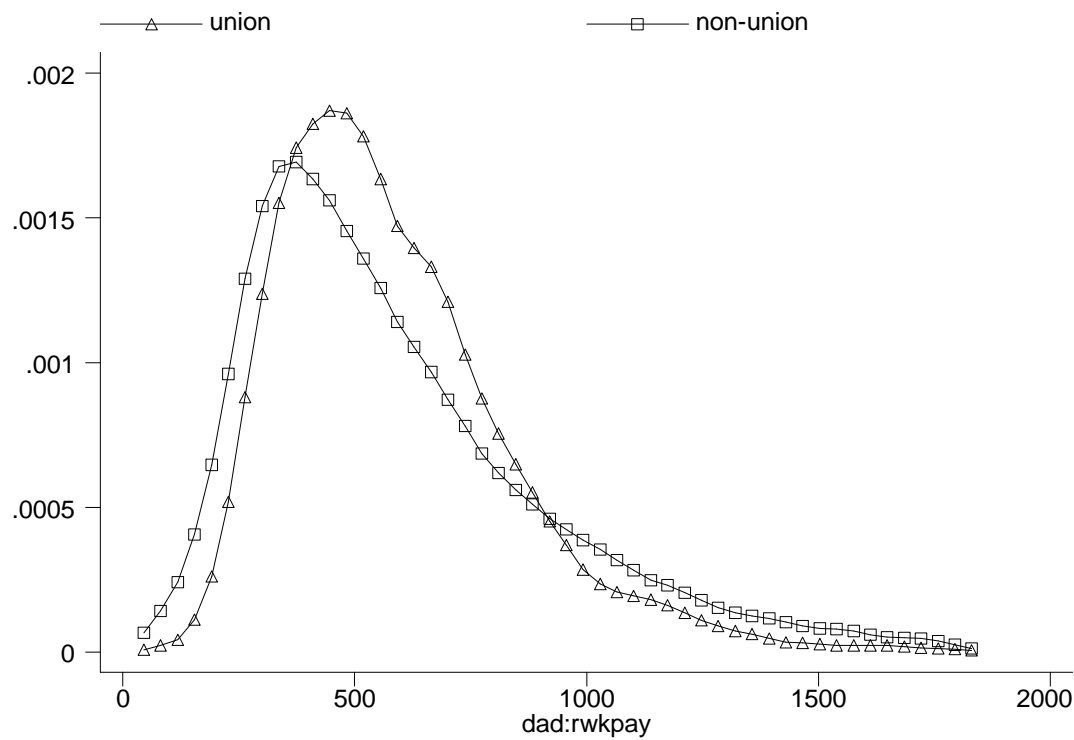
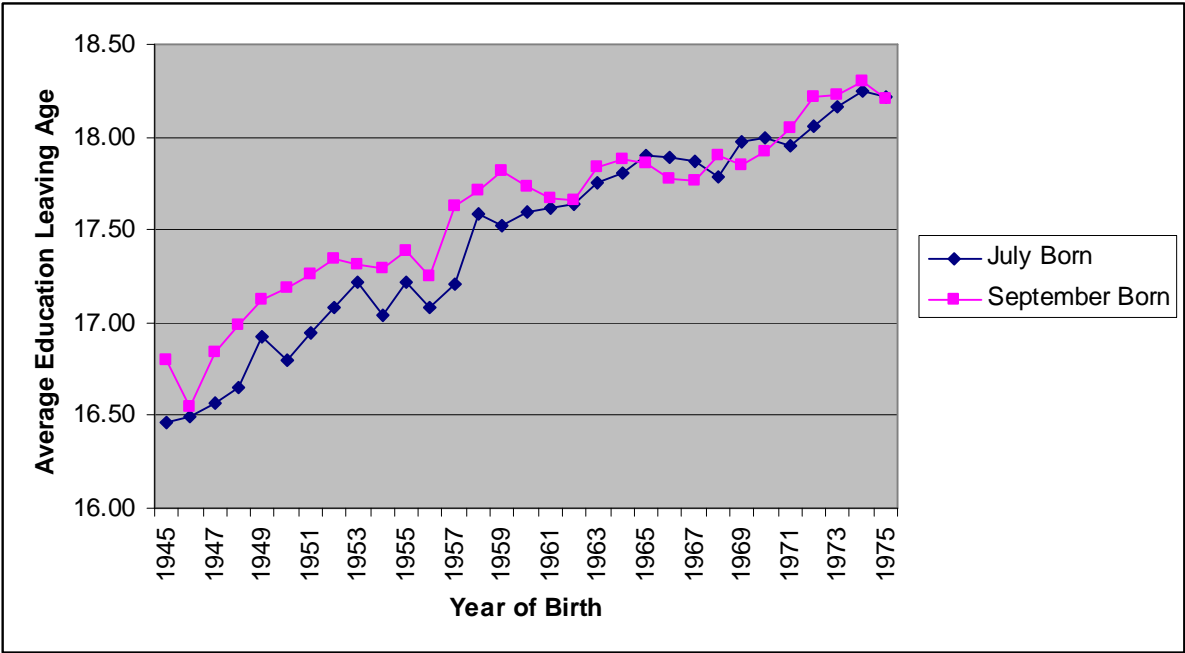


Figure 1. 6 Average School Leaving Age by Year of Birth: England Only



## Tables for Chapter One

**Table 1.1 Sample Selection**

	Living Away from parents	Living with one parent	Living with both parents	Final sample
Age distribution:				
% aged 16	1.99	10.06	10.89	10.29
% aged 17	33.82	47.45	48.94	46.87
% aged 18	64.19	42.49	40.17	42.84
% Staying on at 16	26.99	71.06	75.85	77.53
% Attaining 5+ GCSE A*-C	39.68	67.16	76.54	78.07
Observations:	2,836	9,035	31,103	8,367

Note: The following are dropped from the penultimate column to form the final sample: families where father is not working or self employed or has no or missing reported earnings (approximately nine thousand); families where one or both parents are immigrants (approximately five thousand); very old or very young parents (approximately four hundred); families residing in Scotland and in heavily oversampled Northern Ireland (approximately five thousand); observations missing other information (approximately fourteen hundred); and the bottom 1% and top 5% of the father's earnings distribution (approximately seven hundred).

**Table 1.2 Descriptive Statistics LFS 1992-2006 Estimation Sample**

	Paternal Log Earnings	Paternal School Leaving Age	Maternal School Leaving Age	Paternal Age	Maternal Age	Father affected by RoSLA	Mother Affected by RoSLA	Paternal Union Membershi p	Age of Respondent
<b>Girls: N= 4024</b>									
Did not stay in full time education (17%):	6.04 (0.43)	15.93 (1.48)	15.93 (1.25)	45.41 (5.51)	43.52 (4.84)	0.31 (0.46)	0.23 (0.42)	0.41 (0.49)	17.32 (0.65)
Did stay in full time education (83%):	6.31 (0.50)	17.28 (2.61)	17.17 (2.23)	47.09 (5.08)	45.03 (4.49)	0.26 (0.44)	0.18 (0.38)	0.43 (0.50)	17.32 (0.65)
All	6.27 (0.50)	17.04 (2.50)	16.95 (2.15)	46.80 (5.19)	44.77 (4.59)	0.27 (0.44)	0.19 (0.39)	0.43 (0.50)	17.32 (0.65)
<b>Boys: N= 4343</b>									
Did not stay in full time education (27%)	6.07 (0.45)	15.97 (1.45)	16.00 (1.25)	45.36 (5.04)	43.31 (4.62)	0.34 (0.47)	0.23 (0.42)	0.40 (0.49)	17.35 (0.64)
Did stay in full time education (73%)	6.33 (0.50)	17.49 (2.73)	17.33 (2.32)	47.05 (4.97)	45.10 (4.52)	0.24 (0.43)	0.16 (0.36)	0.43 (0.49)	17.32 (0.66)
All	6.26 (0.50)	17.08 (2.55)	16.97 (2.17)	46.60 (5.05)	44.62 (4.62)	0.27 (0.44)	0.18 (0.38)	0.42 (0.49)	17.33 (0.65)

Note: Selected means, standard deviation in brackets

**Table 1.3 Effects of Parental Education and Income on the Probability of Post-Compulsory Schooling of Children**

<b>Specification:</b>	1	2	3	4	5	6	7	8	9
<b>BOYS: N=4343</b>									
Maternal School Leaving Age	0.033		0.030	0.029		0.028	0.028		0.028
	0.004		0.004	0.004		0.004	0.004		0.004
Paternal School Leaving Age	0.024		0.017	0.014		0.012	0.014		0.012
	0.003		0.003	0.003		0.003	0.003		0.003
Paternal Log Earnings		0.192	0.120		0.111	0.083		0.103	0.076
		0.015	0.016		0.017	0.017		0.017	0.017
<b>GIRLS: N= 4024</b>									
Maternal School Leaving Age	0.025		0.024	0.022		0.022	0.022		0.021
	0.003		0.003	0.003		0.003	0.003		0.003
Paternal School Leaving Age	0.015		0.009	0.007		0.006	0.007		0.006
	0.003		0.003	0.003		0.003	0.003		0.003
Paternal Log Earnings		0.136	0.093		0.064	0.047		0.061	0.045
		0.013	0.014		0.016	0.016		0.016	0.016
Controls for paternal union membership	No	no	no	no	no	No	Yes	Yes	Yes

Control for paternal occupation	No	no	no	yes	yes	Yes	Yes	yes	yes
<hr/> Note: LFS 1992-2006. Standard errors in italics. Specifications 1, 2 and 3 include year of survey dummies, regional dummies, interactions of year of survey and region, dummies of child's date of birth, dummies in the date of birth of both parents in five year intervals.									

**Table 1.4 First Stage Regressions**

N=8,367		1	2	3	4	5	6
Paternal Schooling	Paternal RoSLA	0.270 <i>0.098</i>		0.250 <i>0.099</i>	0.278 <i>0.102</i>		0.258 <i>0.102</i>
	Paternal MoB		-0.015 <i>0.006</i>	-0.014 <i>0.006</i>		-0.015 <i>0.007</i>	-0.013 <i>0.007</i>
Maternal schooling	Maternal RoSLA	0.278 <i>0.08</i>		0.255 <i>0.08</i>	0.284 <i>0.08</i>		0.261 <i>0.081</i>
	Maternal MoB		-0.016 <i>0.006</i>	-0.014 <i>0.006</i>		-0.016 <i>0.006</i>	-0.014 <i>0.006</i>
Paternal earnings	Paternal Union Membership (PUM)	0.072 <i>0.009</i>	0.072 <i>0.009</i>	0.072 <i>0.009</i>	0.049 <i>0.018</i>	0.049 <i>0.018</i>	0.049 <i>0.018</i>
	PUM *Professional				0.009 <i>0.027</i>	0.009 <i>0.027</i>	0.009 <i>0.027</i>
	PUM*Lower Professional				0.005 <i>0.032</i>	0.004 <i>0.032</i>	0.004 <i>0.032</i>
	PUM*Admin & Secretarial				0.102 <i>0.043</i>	0.102 <i>0.043</i>	0.102 <i>0.043</i>
	PUM*Skilled Trade				0.106 <i>0.027</i>	0.106 <i>0.027</i>	0.106 <i>0.027</i>
	PUM*Personal Services				0.304 <i>0.042</i>	0.304 <i>0.042</i>	0.304 <i>0.042</i>
	PUM*Machine Operatives				0.117 <i>0.028</i>	0.117 <i>0.028</i>	0.117 <i>0.028</i>
	PUM*Elementary Occupations				0.208 <i>0.039</i>	0.208 <i>0.039</i>	0.208 <i>0.039</i>
F stats, p values	RoSLA	8.88, 0		7.44, 0	9.00, 0		7.58, 0
	MoB		6.57, 0	5.13, 0.01		6.21, 0	4.79, 0.01
	RoSLA and MoB			7.01, 0			6.90, 0
	Occ				290.14, 0	290.14	290.14, 0
	PUM	59.82, 0	59.82, 0	59.82, 0	7.28, 0.01	7.28, 0.01	7.28, 0.01
	PUM, Occ., PUM*Occ.				187.39, 0	187.39, 0	187.44, 0
	PUM*Occ.				13.34, 0	13.34, 0	13.34, 0
	PUM, PUM*Occ				37.24, 0	37.24, 0	37.24, 0
	Parental Education IVs, PUM	25.8, 0	24.25, 0	17.54, 0	8.43, 0	6.56, 0	6.98, 0
	Parental Education IVs, PUM, Occ, PUM*Occ				166.47, 0	166.1, 0	149.46, 0
	Parental Education IVs, PUM*Occ				12.38, 0	11.77, 0	11.00, 0
	Parental Education IVs, PUM, PUM*Occ				31.58	31.021	27.11, 0

Note: Standard errors in italic. The models also include year of survey dummies, regional dummies, dummies of child's date of birth, dummies in the date of birth of both parents in five year intervals and dummies for paternal occupation. RoSLA is a dummy for the Raising of School Leaving Age, MoB stands for Month of birth (a linear

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function on month with September = 1 to August = 12); PUM is Paternal Union Membership Status; and Occ is paternal occupation (7 categories)



**Table 1.5 Instrumental Variable Estimates: LFS 1992-2006**

Specification:	A	B	1	2	3	4	5	6
Instruments		-	RoSLA	RoSLA Mob	RoSLA	RoSLA MoB	RoSLA	RoSLA MoB
			PUM	PUM	PUM	PUM		
					PUM*Occ	PUM*Occ	PUM*Occ	PUM*Occ
Second stage controls		PUM					PUM	PUM
	Occ	Occ	Occ	Occ	Occ	Occ	Occ	Occ
<b>BOYS: N=4343</b>								
	0.028	0.028	0.078	0.079	0.079	0.079	0.079	0.079
Maternal School Leaving Age	0.004	0.004	0.061	0.060	0.060	0.060	0.060	0.060
	0.012	0.012	0.031	0.023	0.033	0.025	0.039	0.030
Paternal School Leaving Age	0.003	0.003	0.047	0.045	0.046	0.044	0.047	0.045
	0.083	0.076	0.268	0.273	0.338	0.340	0.259	0.266
Paternal Log Earnings	0.017	0.017	0.094	0.094	0.080	0.080	0.144	0.144
<b>GIRLS: N=4024</b>								
	0.022	0.021	0.211	0.184	0.209	0.183	0.208	0.182
Maternal School Leaving Age	0.003	0.003	0.057	0.055	0.057	0.055	0.057	0.055
	0.006	0.006	-0.096	-0.062	-0.075	-0.044	-0.068	-0.037
Paternal School Leaving Age	0.003	0.003	0.045	0.041	0.044	0.041	0.045	0.041

	0.047	0.045	0.233	0.220	0.104	0.095	0.016	0.003
Paternal Log Earnings	0.016	0.016	0.082	0.082	0.072	0.072	0.095	0.094

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Notes: Standard errors in italics. All second stage specifications include year of survey dummies, regional dummies, interactions of year of survey and region, dummies of child's date of birth, and dummies in the date of birth of both parents in five year intervals. RoSLA is a dummy for the Raising of School Leaving Age, Mob stands for Month of birth (linear), PUM for Paternal Union Status, and Occ for Paternal occupation (7 categories)

## Appendix for Chapter One

**Table 1A1 Effects of Parental Education and Income on the Probability of their Children Attaining +5 A\*-C GCSE grades**

Specification:	1	2	3	4	5	6	7	8	9
<b>BOYS: N=3523</b>									
Maternal School Leaving Age	0.030		0.029	0.027		0.027	0.027		0.027
	0.004		0.004	0.004		0.004	0.004		0.004
Paternal School Leaving Age	0.018		0.014	0.010		0.008	0.009		0.008
	0.003		0.003	0.004		0.004	0.004		0.004
Paternal Log Earnings		0.148	0.083		0.076	0.051		0.071	0.047
		0.017	0.018		0.019	0.019		0.019	0.019
<b>GIRLS: N= 3523</b>									
Maternal School Leaving Age	0.026		0.024	0.022		0.022	0.022		0.022
	0.004		0.004	0.004		0.004	0.004		0.004
Paternal School Leaving Age	0.018		0.012	0.010		0.008	0.010		0.008
	0.003		0.003	0.003		0.003	0.003		0.003
Paternal Log Earnings		0.154	0.103		0.086	0.066		0.083	0.064
		0.015	0.016		0.018	0.018		0.018	0.018
Controls for paternal union membership	No	no	no	no	No	No	Yes	Yes	Yes
Control for paternal occupation	No	no	no	yes	yes	Yes	Yes	yes	Yes

Note: LFS 1992-2006. Standard errors in italics. Specifications 1, 2 and 3 include year of survey dummies, regional dummies, interactions of year of survey and region, dummies of child's date of birth, dummies in the date of birth of both parents in five year intervals.

**Table 1A2 Instrumental Variable Estimates: Effects of Parental Education and Income on the Probability of their Children Attaining +5 A\*-C GCSE grades**

Specification:	A	B	1	2	3	4	5	6
<b>Instruments</b>		-	RoSLA	RoSLA Mob	RoSLA	RoSLA MoB	RoSLA	RoSLA MoB
			PUM	PUM	PUM	PUM		
					PUM*Occ	PUM*Occ	PUM*Occ	PUM*Occ
<b>Second stage controls</b>		PUM					PUM	PUM
	Occ	Occ	Occ	Occ	Occ	Occ	Occ	Occ
<b>BOYS: N=4343</b>								
	0.027	0.027	0.094	0.105	0.094	0.105	0.094	0.106
Maternal School Leaving Age	0.004	0.004	0.058	0.057	0.058	0.057	0.058	0.057
	0.008	0.008	0.072	0.034	0.070	0.033	0.088	0.049
Paternal School Leaving Age	0.004	0.004	0.048	0.045	0.047	0.044	0.048	0.045
	0.051	0.047	0.126	0.154	0.191	0.208	-0.021	0.023
Paternal Log Earnings	0.019	0.019	0.109	0.108	0.099	0.099	0.166	0.165
<b>GIRLS: N=4024</b>								
	0.022	0.022	-0.013	-0.010	-0.015	-0.011	-0.016	-0.012
Maternal School Leaving Age	0.004	0.004	0.059	0.058	0.059	0.058	0.059	0.058

Paternal School Leaving Age	0.008	0.008	0.014	0.001	0.034	0.017	0.039	0.021
	0.003	0.003	0.053	0.045	0.052	0.045	0.052	0.045
Paternal Log Earnings	0.066	0.064	0.251	0.258	0.138	0.144	0.079	0.087
	0.018	0.018	0.096	0.095	0.084	0.084	0.110	0.109

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Notes: Standard errors in italics. All second stage specifications include year of survey dummies, regional dummies, interactions of year of survey and region, dummies of child's date of birth, and dummies in the date of birth of both parents in five year intervals. RoSLA is a dummy for the Raising of School Leaving Age, Mob stands for Month of birth (linear), PUM for Paternal Union Status, and Occ for Paternal occupation (7 categories)

**Table 1A3 Effects of Parental Education and Income on the Probability of their Children Staying On (using same sample as those reporting GCSE results)**

<b>Specification:</b>	1	2	3	4	5	6	7	8	9
<b>BOYS: N=3523</b>									
Maternal School Leaving Age	0.023		0.021	0.020		0.020	0.020		0.019
	0.004		0.004	0.004		0.004	0.004		0.004
Paternal School Leaving Age	0.019		0.015	0.012		0.010	0.011		0.010
	0.003		0.003	0.003		0.003	0.003		0.003
Paternal Log Earnings		0.133	0.075		0.068	0.045		0.064	0.042
		0.015	0.016		0.017	0.018		0.018	0.018
<b>GIRLS: N= 3523</b>									
Maternal School Leaving Age	0.020		0.019	0.017		0.017	0.017		0.017
	0.003		0.003	0.003		0.003	0.003		0.003
Paternal School Leaving Age	0.011		0.008	0.006		0.005	0.006		0.005
	0.003		0.003	0.003		0.003	0.003		0.003
Paternal Log Earnings		0.103	0.067		0.046	0.032		0.043	0.030
		0.013	0.014		0.015	0.016		0.016	0.016
Controls for paternal union membership	No	no	No	no	No	No	Yes	Yes	Yes

Control for paternal occupation	no	no	No	yes	yes	Yes	Yes	Yes	Yes
<hr/> Note: LFS 1992-2006. Standard errors in italics. Specifications 1, 2 and 3 include year of survey dummies, regional dummies, interactions of year of survey and region, dummies of child's date of birth, dummies in the date of birth of both parents in five year intervals.									



**Table 1A4 Instrumental Variable Estimates: Effects of Parental Education and Income on the Probability of their Children Staying On (using same sample as those reporting GCSE results)**

Specification:	A	B	1	2	3	4	5	6
<b>Instruments</b>		-	RoSLA	RoSLA Mob	RoSLA	RoSLA MoB	RoSLA	RoSLA MoB
			PUM	PUM	PUM	PUM		
					PUM*Occ	PUM*Occ	PUM*Occ	PUM*Occ
<b>Second stage controls</b>		PUM					PUM	PUM
	Occ	Occ	Occ	Occ	Occ	Occ	Occ	Occ
<b>BOYS: N=3523</b>								
	0.020	0.019	0.068	0.067	0.068	0.068	0.068	0.068
Maternal School Leaving Age	0.004	0.004	0.054	0.052	0.053	0.052	0.054	0.052
	0.010	0.010	0.058	0.049	0.053	0.045	0.059	0.049
Paternal School Leaving Age	0.003	0.003	0.044	0.042	0.043	0.041	0.045	0.042
	0.045	0.042	0.119	0.126	0.206	0.211	0.148	0.158
Paternal Log Earnings	0.018	0.018	0.100	0.100	0.092	0.091	0.153	0.152
<b>GIRLS: N=3525</b>								
	0.017	0.017	0.209	0.194	0.207	0.193	0.205	0.192
Maternal School Leaving Age	0.003	0.003	0.051	0.050	0.051	0.050	0.051	0.050

	0.005	0.005	-0.102	-0.061	-0.083	-0.047	-0.078	-0.043
Paternal School Leaving Age	0.003	0.003	0.046	0.039	0.045	0.039	0.045	0.039
	0.032	0.030	0.208	0.189	0.089	0.078	0.034	0.019
Paternal Log Earnings	0.016	0.016	0.083	0.083	0.073	0.073	0.095	0.095

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Notes: Standard errors in italics. All second stage specifications include year of survey dummies, regional dummies, interactions of year of survey and region, dummies of child's date of birth, and dummies in the date of birth of both parents in five year intervals. RoSLA is a dummy for the Raising of School Leaving Age, Mob stands for Month of birth (linear), PUM for Paternal Union Status, and Occ for Paternal occupation (7 categories)

# **Chapter Two: Money, Mentoring and Making Friends: The Impact of a Multidimensional Access Programme on Student Performance**

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## **2.1 Introduction**

There is a pronounced global socioeconomic gradient in educational attainment, particularly at the university level (see Digest of Education Statistics, 2007 for USA; Eurostudent, 2005 for Europe). Poor attainment by low socioeconomic status (SES) groups limits inter-generational mobility and reinforces socioeconomic inequalities. There are multiple causes for such inequalities, including institutional barriers, low quality schooling, credit constraints, and a lack of parental investment. Recent work has emphasized the higher returns to intervening early in life to improve educational outcomes (Cunha and Heckman, 2007; Heckman, Moon, Pinto, Savelyev, and Yavitz, 2010). Yet a more commonly used policy is targeted intervention programs by universities and colleges to boost the educational attainment of disadvantaged social groups.

While access programs are becoming increasingly diverse in their approach to tackling the barriers to progression to university and promoting educational success, the majority of programs focus exclusively on providing financial supports to students. Thus much of the literature, as demonstrated in a review by Deming and Dynarski (2009), concentrates on the effectiveness of financial aid programs such as the Pell Grants and HOPE scholarships. There are also programs that couple financial aid with other forms of outreach initiatives such as academic and social supports. Yet evidence of the effectiveness of these more multifaceted programs is lacking, with only a few rigorous studies adopting experimental designs or convincing natural experiments (see, for example, Angrist, Lang, Oreopoulos, 2009; Brock and Richburg-Hayes, 2006; Scrivener, Bloom, LeBlanc, Paxson, Rouse and Sommo, 2008).

This study contributes to this literature by using a quasi-experimental design to evaluate a comprehensive university access program (AP) which operates at University College Dublin (UCD) in Ireland. UCD is the largest Irish university with over 20,000 undergraduates and postgraduates and is located in the southern suburbs of the city. The AP is provided to students attending disadvantaged high schools that are linked to the program based on a set of eligibility criteria. The program operates a range of pre- and post-university entry support mechanisms which provide financial aid, as well as academic and social support to the student. The AP differs from many of the US-based programs as aid is unconditional in that the students receive financial assistance regardless of their university grades. In addition, students of mixed ability are admitted to the program. Evaluating the effectiveness of access initiatives targeting disadvantaged students in Ireland is particularly salient as the rate of return to education is higher than in other European countries and is comparable to the US (Trostel, Walker and Woolley, 2002). Furthermore, educational inequality is relatively large in Ireland where, out of twenty OECD countries, the correlation between father's education attainment and their children's education is highest (Chevalier, Denny and McMahon, 2009), indicating that there is a need for policies to improve intergenerational mobility.

There is a well developed literature identifying the effects of financial aid on enrolment to university (for example, Cornwell, Mustard, and Sridhar, 2006; Kane, 2003; Dynarski, 2003). The magnitude of this effect is typically around a 5% or less increase in enrolment for a \$1,000 reduction in student costs (Deming and Dynarski, 2009). Financial aid can also have a positive effect on university completion rates and graduating on time (e.g. Scott-Clayton, 2009; Dynarski, 2000). There is also some evidence that academic

support programs, which do not provide financial aid, can be effective. Lesik (2007) finds a positive relationship between a remedial mathematics program and student retention using a regression discontinuity design. In addition, Scrivener et al. (2008) identify a positive treatment effect on first semester academic performance in an experimental evaluation of the Open Doors program in a US community college which provides improved counselling and monitoring of students. Finally, the Upward Bound project is the longest running federal US program which provides additional academic and social services to disadvantaged students during high school. This program, which resembles many of the characteristics of the AP discussed here, was evaluated using an experimental design and found that it had limited effects on both high schools outcomes and post secondary school outcomes including progression to university and earning a degree (Myers, Olsen, Seftor, Young, and Tuttle, 2004; Seftor, Mamun, and Schirm, 2009).

Yet there have been relatively few studies that examine multidimensional programs such as the AP discussed here, that combine financial aid with academic and social supports both prior to and during university. Exceptions include Angrist et al. (2009) which conducted an experimental evaluation of the Student Achievement and Retention (STAR) project in a Canadian university. Students were randomly assigned to three groups which received academic support, financial incentives or a combination of the two. The program reduced the probability of first year withdrawal by 10% and had positive effects on GPA. These effects were greater for students who received the combined financial and academic supports package. However, the effects were only statistically significant for female students.

Another study by Brock and Richburg-Hayes (2006) evaluates the impact of a Louisiana needs-based scholarship program on course completion and exam performance of low-income parents attending community college. Students were randomised into a treatment and control group. In addition to the regular financial aid received by the control group, the treatment group students were provided with scholarships of \$2,000 per annum if they attended at least half-time and attained, on average, a C grade. While both groups could avail of counselling services, the treatment group were obliged to attend student counselling in order to receive the financial aid. The program had multiple positive effects. In particular, the treatment group were more likely to be full-time college students, passed more college courses and earned more credits, and were more likely to register for their second and third years of college.

In addition to the financial, academic and social elements of the Irish access program, another key aspect which distinguishes it from the STAR project for example, is that preferential entry to university is provided to students i.e. some AP students enter university with grades that are lower than the regular minimum grades necessary to be offered a place at university. Although this preferential treatment is not based on ethnicity, there are parallels with US and Indian affirmation action (“positive discrimination”) programs (Deshpande, 2006). Affirmative action programs based on ethnicity have proved controversial in recent years (see Fryer and Loury (2005) for an interesting discussion). One criticism is that race-based affirmative action is seen to favour economically well-off minority students. This has led to calls for the ethnicity criteria to be replaced by socio-economic criteria. The Irish AP, which is based on socio-economic criteria alone, may

therefore be informative for policymakers considering a switch away from such race-based criteria.

In the absence of a randomized control trial, our analysis relies on a natural experiment which exploits the gradual and non-systematic expansion of the program over time. The identification strategy compares students from high schools which were chosen to be part of the program in the early years to those that were chosen to join the program in later years. As there was no systematic difference in the characteristics of the high schools which joined the program at different times, a comparison of students from these schools allow us to identify the treatment effect. Our analysis examines both first and final year outcomes including exam performance and graduation rates. In addition, we model the impact of the program on final degree classification which is often overlooked in the literature, despite some studies finding a high rate of return to university grades (see Jones and Jackson, 1990; Schweri, 2004; Bratti, Naylor and Smith, 2007).

Overall we find that the program has positive effects on first and final year outcomes, however unlike the Angrist et al. study, we do not identify any gender effects. We find similar patterns of results for treated students that entered through the normal entry system and the ‘affirmative action’ group i.e. the treated students entering with lower grades. Note that the analysis is based on university administrative data, therefore all results are conditional on the student having applied and been accepted into the university.

The paper is organised as follows: Section II describes the access program in detail. Section III discusses the methodology employed and data used in the analysis. Section IV



presents the main results of the analysis and a description of the sensitivity analysis conducted. Section V discusses the results and concludes.

## **2.2 Description of the Access Program**

The access program has been operating at UCD since 1997 and aims to increase university participation and improve the academic performance of students from socio-economically disadvantaged backgrounds through a range of pre- and post- entry support mechanisms. Much of its pre-entry activities involve outreach activities with disadvantaged schools at both primary (4-12 years) and second level (12-18 years) which focus on raising student aspirations and creating an awareness of further education. These activities include field trips to the university where students attend sample lectures, participate in science labs, as well as a variety of sports and social activities. The AP also organizes pre-entry orientation programs and shadowing days where high school students follow a university student through a day at university. The program also provides direct academic support to high school students for the final state exam in the form of one-to-one tuition and revision workshops. At a community level, the AP gives presentations to parents and contributes to community-based events. The number of pre-entry activities provided to schools varied overtime, with an average of three activities in 1999 and 2000, seven in 2001 and 2002, and six in 2003 and 2004.<sup>21</sup> The aim of these pre-entry activities is to increase the number of applications to university by disadvantaged students.

The AP also provides information to students about its alternative entry mechanisms into university. The regular Irish university admissions system is a nationally

<sup>21</sup> The activities include: shadowing days, voluntary tutoring, Take 5, Uni 4 U summer school, 5<sup>th</sup> Year summer school, achievement awards, educational funding, Uni in Community, Leaving Certificate exam workshop, Discovering University, Discovering Maths, and the HEAR Scheme.

administered clearing mechanism based on supply and demand for university places across all third level institutions. Prior to taking the final state exams at about age 18, high school students rank their top ten preferred degree courses. Their chosen courses may be at different universities and/or be different courses at the same university. Several months later they sit their final state exams. These exam grades are converted to a points-scale from 0-600 in increments of 5 points and are used to rank the students. Offers for a place on a particular course at a particular university are made to the highest scoring students who applied for that course at that university.<sup>22</sup> Further offers are made on the basis of grades until all places have been filled. Students who are not offered their first ranked course are then considered for their second ranked course and the process continues until all places are allocated. The supply of places on degree courses seldom changes from year to year. The minimum points necessary for a place on a course, which is set by the grades of the last person admitted, can fluctuate from year to year.

Under the AP, two types of students are treated. “Merit Treatment” students are admitted to university through the nationally administered admissions system described above. About 45% (from a total of 100-140 students per annum) of AP students attain

<sup>22</sup> Offers are made on the basis of actual grades attained in the final state exams and not on grades predicted by teachers, etc. Applications are anonymous and personal statements, references from teachers, subject specific aptitude tests or interviews are not used in this system. See Gormley and Murphy (2006) for a more detailed description of the university admissions system in Ireland.

sufficient grades to meet the minimum points level required for regular university entry and are allocated a place on their preferred course in the usual manner.

“Discount Treatment” students (the remaining 55% of the total) receive preferential treatment in attaining their university place such that they receive a concession of up to 20% on the competitive entry points for the course set by the national admissions system. Thus a certain number of places on each course are reserved for students who do not meet the minimum points level required for that course. To be offered one of these places they must meet certain basic requirements (e.g. a medical student must have studied science at high school) and provide further information regarding their socio-economic circumstances (discussed below) as well as references from their high school teachers<sup>23</sup>. The number of minimum reserved places on each course is based on the size of each faculty in the university and is relatively fixed. If there is a surplus of suitable and eligible applicants for these places, the limited places are awarded on the basis of points attained in the final state exams.

Note that AP students are not guaranteed a place in the university. Essentially, the number of points they receive in the final state exam and their preferred course choice determines whether they are classified as Merit or Discount students. Discount students are not necessarily of a lower ability than Merit students, rather, they may have applied for a course that required higher minimum points.

<sup>23</sup> These references are only considered in tie-break situations i.e. where two or more students with the same points are competing for a place on the same course. Therefore, this subjective information is not used by the AP office in the majority of cases.

Post-entry, both Merit and Discount AP students receive the same supports. Students receive an extra top-up grant which supplements the regular means tested government grant<sup>24</sup> and in most cases, doubles the amount of financial aid they would otherwise receive. This grant totalled between €2,200 (US\$3,236 in 2008 prices) and €3,400 (US\$5,000 in 2008 prices) per annum during the period under analysis. In addition, they receive book vouchers and course materials such as laptops, lab coats, etc. The AP students are also provided with a number of post-entry supports geared towards liaising with students once they have commenced their studies.

The AP students also participate in a pre-term orientation week where they live on campus with other AP students to encourage early social and academic integration. The AP students can also avail of social supports, if required, from student advisors in the AP office. Such socialization services, which encourage students to interact and ‘make friends’, are important as there is evidence that strong social networks are associated with increased college retention and academic performance, particularly for under-represented college students (Tinto, 1993; Fischer, 2007; Eggens, van der Werf, and Bosker; 2008). Finally, to help maintain their academic standards while at university, they may also receive free additional tuition, if required, in the form of one-to-one and group tutorials.

<sup>24</sup> All AP students are in receipt of the means tested grant from central government which is valued at €2,900 (US\$4,265) and €3,300 (US\$4,854) per annum in 2008 prices. This scheme is called the ‘Higher Education Grant’ and is administered by Local Authorities. The amount of the grant is dependent on family income, number of siblings and distance to the university. It is provided to all eligible students who are undertaking full time undergraduate or postgraduate courses (for more information see Clancy and Kehoe, 1999). On average, almost one third (~32%) of all university students in the country were in receipt of the grant during the period under analysis (Department of Education and Science Statistical Reports, 1997-2005).

To be eligible for the program, both Merit and Discount students must meet four criteria. First, eligibility is means tested such that parental income has to be below a certain threshold. This threshold is the same as that of the regular means tested grant which any student can apply for. As family income is not observed in the data, one of the selection criteria for choosing the Control group is based on receipt of the regular grant. Second, in order to be eligible for the AP, neither parent must have graduated from a university. Third, the student's parents must be a member of the following socio-economic groups: unskilled manual, semi-skilled manual, skills non-manual, and non-farming agricultural workers.<sup>25</sup> Students whose parents are professionals, employers or managers are not eligible for the AP. As measures of parental education are not available in the data, socio-economic status is used as a proxy for parental education.<sup>26</sup> Finally, the student must be attending a high school which is designated as 'disadvantaged'. This criterion is key to the identification strategy and is discussed in detail below.

<sup>25</sup> There is some evidence that farmers and self employed people circumvent the rules on grant eligibility (Department of Education, 1993), therefore by including these students we could have Control students who are better-off financially than the Treatment group. Furthermore, as rural schools tended to join the program at a later stage, we do not wish to conflate the effect of coming from a rural background with that of the program. Therefore farmers are excluded from the Control group. It is not possible to identify self employed people using the socioeconomic categories observed in the data.

<sup>26</sup> It is possible that there are parents with university level education in the remaining social-economic groups (i.e. unskilled manual, semi-skilled manual, skills non-manual, and non-farming agricultural workers), although we assume that this is not the case in general.

## **2.3 Estimation & Data Issues**

### **A. Identification Strategy**

The identification strategy is similar to that of Lavy and Schlosser (2005) which relied on the expansion in the number of schools participating in a remedial educational program in Israel. Hence the identification strategy exploits the gradual, and non-systematic, expansion of the access program into high schools over time.

The key eligibility criterion of the AP is whether a student attended a disadvantaged high school linked to the AP prior to entering university. When the program began in the late 1990's, certain schools were identified from the Government's list of officially designated disadvantaged high schools and became linked to the AP. Schools are included on the Government's list based on a range of socio-economic and educational indicators such as local unemployment rates, measures of poverty, and information on basic literacy and numeracy levels in the locality. Over time more schools from this list were linked to the program when funding allowed.<sup>27</sup> The data available for the analysis covers students entering the university between 1999 to 2003, therefore schools linked to the AP in 1999 or before represent the "always" covered group. Those schools that were included in the program for the first time in 2004, or after, represent the "never" linked group.<sup>28</sup>

<sup>27</sup> No school which joined the access program has been dropped or exited from the program.

<sup>28</sup> The period covered by the data could not be extended in either direction due to changes in data storage systems and the adoption of a new North American style GPA system to replace a traditional British style grading system in 2005.

Essentially, the analysis compares the treated students who participated in the AP, to students who met all of the other eligibility criteria discussed above, except their schools had not yet become linked to the AP at the time they applied to university. However, the high schools which these students attended eventually became linked to the program. The Treatment group is therefore all Merit and Discount students who attended a disadvantaged school linked to the program and entered the university between 1999 and 2003. The Control group consists of students who were in receipt of a state grant, members of one of the six identified lower social-economic groups and attended a disadvantaged school which was not linked to the program at university entry, but later became linked to the program.

## **B. Assumptions of the Identification Strategy**

The identification strategy is based on the assumption that there was a random selection of schools into the program, thus the date at which schools became linked to the program must not depend on the characteristics of the school. If there was a non-random selection of schools, this may bias the results as the Treatment group (those who joined earlier) and the Control group (those who joined later) may systematically differ. However for several reasons we believe this is not the case. First, there was no self-selection of the schools into the program as the schools were chosen by the AP to join the scheme. In regards the expansion of the program, when it first began the AP worked with schools within a defined catchment area, and when funding allowed, the program expanded to include new schools chosen from the Government's list. The expansion of the program to new schools was dependent on funding from the Irish government, the European Union and



from philanthropic bodies, and as such, can be viewed as an exogenous source of variation in the treatment group.

It is important to examine the geographical distribution of the schools which joined the program at different time points as evidence suggest that the costs of attending university are lower for students who live closer to the university and that residing near a university is correlated with ability (Card, 1995, 2001). At the start of the program each of the country's seven universities operated access programs independently from each other in terms of the admissions process. The defined catchment areas of each Dublin university included both urban areas and a rural area in another part of the country. For example, UCD was linked to urban disadvantaged high schools located in south west Dublin and to rural schools in the south-eastern area of the country. Other Dublin universities, such as Trinity College Dublin and Dublin City University, were linked to schools in other parts of Dublin city, as well as certain rural areas spread throughout the Republic of Ireland. It is clear from Table 2.1, which shows the number of high schools joining the program over time by commuting distance from UCD, that the treatment and control groups includes students originating from schools which are within commuting and non commuting distance to the university. Therefore there are students joining at different time points who face similar

costs in attending university, as measured by the distance of their former high school to the university.<sup>29</sup>

In addition, Figures 2.1 and 2.2 report the geographical location of the high schools, within Ireland and Dublin respectively, by the year they became linked to the UCD program between 1997 and 2007. It highlights the random nature of the location of linked schools by year of entry. These figures clearly show that there were schools located in both urban and rural areas at the start of the program, and as the program expanded over time, additional schools from both urban and rural areas became linked to the access program.

A further source of exogenous variation in the expansion of the AP was the introduction of a national access scheme in 2001 to co-ordinate the allocation of places for access students amongst nearly all Irish universities. Prior to 2001 students attending schools linked to one institution could only avail of that university's access program. However they could attend the other universities but not receive any benefits from the access program. In 2001 the universities harmonized their admissions schemes. Therefore students from schools in the catchment area of other universities could apply for the AP at UCD. Essentially this policy change linked 125 new schools to the AP. Note however these schools did not receive the pre-entry supports.<sup>30</sup> This major administrative change occurred

<sup>29</sup> We define commuting and non-commuting distance to the university using the Government's definition of commuting distance which is used as a criterion for allocating the level of regular means tested government grant. At the time of the study, a distance from home of less than 24kms was defined as a commuting distance.

<sup>30</sup> Although they may have received pre-entry treatment by the university which was originally linked to their school. The majority of Irish access programs offer similar pre-entry supports to those provided by this AP.

during the period covered by the data and represents an exogenous policy change that greatly expanded the number of linked high schools.

Finally, there was little overt heterogeneity in the quality of high schools linked to the AP. The Government's list from which the schools are drawn is not a ranking and thus each school is regarded as being equally disadvantaged in that they all receive the same level of additional government funding compared to regular schools. In sum, these factors reduce the likelihood that the schools which joined the AP at different times were systemically different.

To provide evidence that the date on which a school joined the AP is not a function of its individual characteristics e.g. school quality, Table 2.2 presents two local labour market characteristics of the electoral district (EDs) of the link schools prior to them becoming linked.<sup>31</sup> As these figures represent Census data at a very detailed level of disaggregation, they are capturing very local neighbourhood effects.<sup>32</sup> The table indicates that there is no clear relationship between the characteristics of the school's neighbourhood and the year in which the school joined the program. Apart from 1999 when the local unemployment rate and proportion leaving school before age 18 is higher than subsequent years, there is no systematic variation in the rates over time. This suggests that Control group students did not attend schools in neighbourhoods that are significantly better or worse than the treated schools which joined the AP earlier.

<sup>31</sup> School quality data are not available to the researchers, therefore local labour market and educational attainment data is used as a proxy.

<sup>32</sup> There are 3440 EDs in Ireland representing 70,280 square kilometres, thus the average size of an ED is 20.4 sq km.

### **C. Self Selection**

Normally a student must have attended the same linked school for a total of five years before applying to the university to avail of the access program; however a very small number of exceptions are made for recent immigrants or returned emigrants. Despite this, there may be a concern that parents chose to send their children to high schools that were linked to the program in order to avail of the AP supports, and the alternative entry mechanism in particular. In Ireland, there are no geographical restrictions on school choice, so in principle, there could be self-selection into an AP linked school if, for example, families who send their children to an AP linked schools have some unobservable characteristics which also affect student outcomes. A priori this is unlikely for two reasons: firstly, AP schools are typically clustered into disadvantaged neighbourhoods, therefore the switch to an AP school would require the student to travel a significant distance to another school, secondly, many low SES parents are unaware of the AP status of the school prior to school entry, as much of the AP supports do not begin until the later grades at high school.

Another potential self selection issue is that as we only observe students who attend UCD, the analysis is conditional on enrolment. As a consequence the Control group, who are socio-demographically similar to the Treatment group, may be a self-selected group as they chose to attend university without the safety net of the access program. Such students may perhaps be unobservably more able or more motivated. Table 2.3 reports the average final state exam grades for the Treatment and Control students. While the grades of the Control students are slightly higher than the Treatment students, the difference is only statistically significant in one of the five years. In addition, there are no systematic changes

in the ability of either the Treatment or Control group over time, suggesting that the composition of the groups are not changing.

Figure 2.3 shows the university faculty of the Treatment and Control groups before and after the introduction of the national access scheme in 2001. It shows that firstly, there is little difference between the faculty choice of the Control group before and after 2001 suggesting that the introduction of the national access scheme did not change the faculty choice of the Control group, and secondly, there is little difference in the faculty choice of the Treatment and Control groups, with slightly more AP students studying Commerce and Law after 2001.

While these data suggest that the Control group do not differ in quality, over time, in any observable way, it is still possible that they may differ with regard to unobservables. If this is the case, the treatment effect may be an underestimate of the true treatment effect. To account for this, we control for both ability (final state exam grades) in our models.

#### **D. Pre- and Post-entry Effects on the Treatment Group**

Another related issue is that we do not observe the initial pool of applicants to the university. It is possible that both grades in the final state exam and faculty of choice are influenced by the pre-entry supports provided by the AP (such as the outreach activities and academic support). Hence there may be a correlation between unobservables that affect the outcome and the probability of treatment. As we only evaluate university based outcomes, such as exam performance, it is conceivable that the results may be driven by such selection effects in either a positive or negative direction. On the one hand, the pre-entry supports

may increase the student's state exam grades, which are required for entry into the university, either directly (through additional tuition) or indirectly (by improving motivation). On the other hand, there could be a complacency effect in that the student reduces their effort in their state exam in the belief that they may be able to enter university with lower required grades. The impact of this selection effect on the results will depend on which effect dominates. It is also possible that the pre-entry supports were not effective at improving entry exam grades and thus progression to university. In this case, any observed treatment effects on university outcomes could be attributed to the post entry supports and selection into university would not be an issue.

As the distribution of the pre-entry supports provided by the AP to the linked schools is not uniform across all linked schools we can investigate these potential effects. Some schools receive more pre-entry activities than others, while some schools only receive the alternative entry mechanism support. In general, schools that are located in urban areas receive more supports than the rural schools. In addition, the number of services provided to schools typically increased over time. To investigate the impact of variation in pre-entry supports, we estimate our results separately for students that attended schools which received full pre-entry support and those that attended schools which only received limited pre-entry support.

## **E. Identifying Suitable Controls for Discount Students**

As we are comparing Discount students to Merit students who have higher final state exam grades, a potential concern is that our Treatment and Control groups are not

comparable. However this is not necessarily the case as there is an overlap in the support of the entrance exam grades for these groups. Table 2.4 shows that the distribution of exam grades intersects for Control students and all but the lowest achieving Discount students. In some cases we have Control students with the same exam grades as Discount students in the same course but who entered the university in a year where the minimum points level had been lower. For example, there are Discount students who entered the Agricultural Science degree in 2001 with 320 points when the minimum required for the general student body was 330, and the following year a Control student entered with 320 points as the minimum required had fallen to 310. In addition, Figure 2.4 shows the distribution of grades in the final high school state exam for Discount, Merit, and Control group students. It shows the similarity of the Merit and Control group students, which largely follows a normal distribution, while the Discount students are skewed to the left.

There are also Control students with the same points as Discount students who entered in the same year but are taking courses with lower minimum entry requirements due to those courses being of lower demand in a particular year. One could argue that these lower demand courses may be of a different difficulty level - however we also estimated the models including the student's faculty to control for the inherent difficulty of the course; however the results are very similar to our base results which do not control for faculty.

This is particularly an issue for the Arts degree course, which is the lowest entry points course and the largest course, as there are Discount students who have lower points than anyone else in the university and few Control students with a similar level of points. Table 2.4 shows that, in particular, there are large differences in the number of Discount and Control students in the lowest points category. While this will not affect the results for

higher achieving student (>400 points), it may downwardly bias the results for the low point students if we are comparing the low point Discount students to Control students who mostly have higher points.

## **F. Data & Method**

We use pooled cross sections of student level administrative data from the university's admissions and grading systems which contains information on all students entering the university from 1999 to 2003 inclusive.<sup>33</sup> The data contain information on student outcomes at university, pre-university academic performance, high school attended, grant status, the student's age, gender, treatment status, and markers of AP eligibility such as the socio-economic group of the student's family. Some school-level information was matched to the individual student-level data using a school identifier. For example, census information on average years of schooling and unemployment rates in the electoral district of a particular school were included. School level information regarding exam results and other school "quality" variables could not be included as this information is not available to researchers in Ireland. Descriptive statistics on the socio-demographic and academic

<sup>33</sup> The working sample excludes those with missing data and students who bypass the regular admissions system for any reason including disabled students, students entering university later in life, as well as overseas students. Students who died during their time at the university have also been excluded from the analysis.



characteristics of the students broken down by the general study body, Control group students, and Discount and Merit AP students are provided in Table 2.4.

The table also reports the results of balancing tests comparing the Control group to the Discount group and the Merit group respectively. It shows that there are no statistical differences between the groups in regards to gender and grant status. However the Discount group does differ from the Control group with respect to the proportion of fathers working in non-manual and manual employment, with more Control group fathers working within the manual sector than the Discount students' fathers. The groups also differ with regard to their grades in the final state exam, with the Discount students typically receiving fewer points than the Control students, and the Merit students following a different distribution of points than the Control students. Finally, due to the identification strategy, the Control group students also typically entered university in different years to the Treatment group students. To control for these differences in student characteristics, all models control for year of university entry, gender, and number of points attained in final state exams.<sup>34</sup> In addition, the distance from the high school to the university in kilometres, local unemployment rates and education levels in the locality of the high school, are also included to control for potential selection effects. Only the results for the main outcomes are presented.

<sup>34</sup> Throughout these models we have controlled for points in the final state exam linearly, however the pattern of results holds when controlling for exam points using different non-linear functions.

The outcomes of interest measure different facets of academic performance in the first year of university as well as the overall performance of the student in their degree course.<sup>35</sup> The first and final year outcomes are categorical variables which are defined as receiving an honours grade, a pass grade, or failing/dropping out of university in either first year or final year respectively.<sup>36</sup> An ‘honours’ grade is defined as receiving a first class honours, a second class upper honours, or a second class lower honours, which is the traditional grading system used in Ireland and Britain rather than using the exact grades or a North American-style GPA system. A comparison of the British/Irish and North American grading system is provided in Appendix Table 2A1. A ‘pass’ grade is defined as receiving a pass grade. Finally, ‘fail/dropping out’ is defined as either failing the exams or dropping out of university. Note that the fail/dropping out category for first year outcome includes students who dropped out before sitting their first year exams, as well as those who sat the first year exams and failed. For the final year outcome this category includes those who dropped out of university at any point in time during their degree program. Table 2.5 compares the outcome variables broken down by the general study body, Control group students, and Discount and Merit AP students. In general, the Discount students perform

<sup>35</sup> We do not to examine outcomes in the second year, third year, etc., of a degree course as different courses are of different durations and some courses use pre-final year exam results for final degree grades whereas others do not. In addition, the majority of dropping-out (~80%) in Ireland occurs between first and second year, which is a far higher than the UK figure of 56% (Smith and Naylor, 2001).

<sup>36</sup> Unfortunately it is not possible within this data to distinguish between students who repeat years because of failing exams, illness or by choice.

worse than the Control students, while the Merit group outperform the Control group. The analysis below will test whether these differences can be attributed to the AP.

Rather than using a standard ‘differences in differences’ method which would require controlling for school fixed effects by including dummy variables for each school, we estimate a simple ‘differences’ model. This issue arises as there are over 300 linked schools involved in the AP and only 168 students in the Treatment group. While we report differences-in-differences results in the sensitivity analysis we do not report them as the main results as the relatively small sample size makes these estimates necessarily imprecise.<sup>37</sup> Ordered probit is used to model the two main outcomes and marginal effects are reported.

<sup>37</sup> Due to the nature of the program some schools only send a small number of students, if any, in a given year to the university.

## 2.4 Results

The impact of the access program on first and final year student outcomes is presented in Table 2.6. The analysis is conducted for all AP students in models (1) and (3) separately for Discount and Merit students in models (2) and (4), by including an interaction term for treatment type. The same Control group is used in each analysis. The first year outcomes are reported in the upper half of the table and the final year outcomes are reported on the bottom half of the table. It shows that the AP has a positive effect on first and final year outcomes. Model 1 shows that AP students are 12% more likely to receive an honours grade and 7% less likely to receive a pass grade. In addition, being in the program reduces the probability that the AP students will either fail or drop out during their first year at university by 5%. Model 2 indicates that the results are similar for both types of treatment students. The AP increases the probability of receiving an honours and reducing the probability of failing/dropping out in first year for Discount students, and increases the probability of receiving an honours and reducing the probability of receiving a pass grade or failing/dropping out in first year for Merit students. The sizes of the effects are similar for both Discount and Merit students and a Wald test reveals that the coefficients are not statistically different from each other ( $\chi^2=0.02, p=0.898$ ).

Model 3 shows that the impacts of the program extends beyond first year, such that the AP increases the probability of receiving an honours degree by 8%. It also reduces the probability of receiving a pass degree by 4%, while reducing the likelihood of failing or dropping out of university by 5%. Thus the program improves the overall graduation rate for AP students. Model 4 suggest that these effects are primarily driven by Merit students,

as the AP increases the probability of receiving an honours degree by 10% and reduces the probability of passing or failing by 4% and 6% respectively, while having no impact on the final degree outcomes for Discount students. However a Wald test indicates that the coefficients for Discount and Merit groups are not statistically different from each other ( $\chi^2=0.21$ ,  $p=0.647$ ). Thus, as the program appears to have the same impact on Discount and Merit students, we only examine the impact of the program for all treatment students in all further analyses. Overall, the program has long term effects in that it helps the student reach the final year, as well as improving their overall degree performance.

For the vast majority of university courses, the first year does not count for the overall degree classification. However the grades from individual first year courses might be considered by employers whilst the student is in final year and conducting a job search whilst their final degree classification is yet to be decided. It could also be the case that the program is changing the students' first year goals. They may feel that they "owe" it to the course administrators to get as high a mark as possible in first year or the program may convince students that getting as high a grade in first year is intrinsically good for some other reason. Control students might be content to just pass first year and not attain high grades. Thus the effect of the program in first year may reflect this "goal setting" effect. However we find that the program still has significant effects on overall degree performance although the point estimates are smaller than the effects on first year outcomes.

## **A. Robustness, Sensitivity, and Extensions**

Table 2.7 reports the results from eight additional analyses which test the robustness and sensitivity of the main results. The first six models investigate whether the program has differential effects across different groups of students by including interactions for low and high ability students, male and female students, and students originating from schools within commuting distance to the university or not. The first year outcomes are reported in the upper half of the table and the final year outcomes are reported on the bottom half of the table. Models (1) and (2) report the main effect of the AP on student outcomes and an interaction effect to examine whether the program benefits high or low ability students. Students who attained 400 points or less in the final state exam are classified as low ability students, and those who achieved more than 400 points are classified as high ability students. 400 points is roughly the 75<sup>th</sup> percentile of attainment in the final state exam taken by school leavers and is sufficient to enter the two largest faculties in the university, Arts and Science, with more prestigious courses like Law and Medicine requiring well in excess of 500 points. The results indicate no interaction effects suggesting that the AP has the same effect on low and high ability students.

Models (3) and (4) test whether the AP has differential impacts by gender as found by Angrist et al. (2009). Overall, we find that the AP has no differing effects on males and females for either first or final year outcomes. While the main effects remains statistical significant, the interaction effects are not. These results differ from the STAR program, which is similar in some respects to the AP considered here, yet it only found effects for females.

To address the Card argument (1995, 2001), which suggests that the costs of attending university are lower for students living closer to the university, models (5) and (6) include an interaction term indicating whether the student's high school was within commuting distance to the university. Again, the main effects of the AP on student outcomes remains, however there are no interaction effects, suggesting that distance to the university does not influence the impact of the program on students' outcomes.

While the main analysis does not control for school fixed effects due to the large number of dummy variables that would need to be included in the analysis, models (7) and (8) reports the results of the estimation including approximately 160 high school dummies. While the results are not statistically significant, possibility due to the reduced sample size, they are consistent with the main analysis in terms of their size and direction.<sup>38</sup> As an alternative to using school dummies to pick up school fixed effects we could alternatively use some measure of school quality. However school quality variables are not publicly available in Ireland therefore it is difficult to ascertain the heterogeneity in the quality of

<sup>38</sup> An underlying assumption of the analysis is that the average change in the outcome is presumed to be the same for both the Control and, counterfactually, for the Treatment group if they had not participated in the AP. We are currently assuming that the school level inputs are constant overtime. However, as discussed above, data on the quality of schools are not available for Ireland, therefore we cannot verify this assumption. For a violation of this assumption to bias our results greatly, the quality and distribution of school level inputs would have to have changed significantly in a short period of time (within 5 years). While we cannot observe this directly, it is unlikely to be the case.

schools linked to the program. In one of the few sources available to us, The Sunday Times Guide to Secondary Schools in Ireland (Murphy and McConnell, 2006), nearly all of the ~300 schools<sup>39</sup> linked to the AP are in the bottom 300 places when ranked by the proportion of students that enrol in university.

A number of additional alternative specifications were considered. For example, we did not find the treatment effect to vary by the faculty of the student. Furthermore, we investigated the existence of peer effects in relation to the program; however we could not identify an effect based on having a high proportion of fellow AP students in a particular course or students from a similar social background. For all of these alternative specifications, the sample size may inhibit the detection of an effect.

## **B. Impact of Variations in Financial Aid Package**

Unlike Angrist et al. (2009), our natural experiment does not allow us to identify the relative effects of the individual financial, academic, and social supports. However variation in the levels of financial aid over time allows us to identify the effects of changes in aid on student outcomes. The amount of financial aid made available to each student changed during the period covered by the data due to funding availability which is exogenous. Furthermore the value of the regular state means tested grant, which all AP

<sup>39</sup> Not all of the linked schools sent students to UCD in the study period. The Sunday Times Guide is only available for 2004/2005, therefore it is not possible to examine variation in the university participation rates for the schools before and after they became linked to the AP.



students additionally receive, also changed over time. The sum of the total value of the AP's aid package was particularly high in 2000, 2001 and 2003 with an average of €6,313 (expressed in 2008 prices) per annum. While the average in 1999, 2002 and 2004 was relatively lower at €5,407 per annum. Therefore there were substantial variations in aid across time for students during their first year of study. As there was not much variation in the total aid package received by student over the entire degree program, only first year outcomes are examined here.

To determine the impact of changes in financial aid on first year exam results, Table 2.8 presents an ordered probit model including an interaction indicating whether the student entered university in a "higher value" year relative to entering in a "lower value" year. Although the estimated results follow a pattern suggesting that the extra funding was beneficial, the first year outcomes for students who received the high value package were not statistically different from the students who received the lower value package. Furthermore no significant effects of the extra funding were detected when alternative models were estimated. Clearly this does not suggest that the AP's financial package has no effect on student performance, however it does imply that increasing the value of the package from an average of €5,407 to €6,313 per annum did not lead to changes in student achievements. The analysis rests on the assumption that there were no other differences in the AP's activities in these high value years that may influence outcomes. It also assumes that the unobserved characteristics of students in the high value years did not differ from students in low value years.

### **C. Selection Effects**

As discussed above, one may argue that the AP's pre-entry treatment may be changing the pre-entry academic achievements of the students. The pre-entry activities, such as summer schools and extra tutorials, may directly improve performance in the final state exam, thus upwardly biasing the treatment effects observed above. Information regarding the number and type of pre-entry activities is available for all UCD linked schools in each year. A school which received three or more activities is considered a full pre-entry supports school, while a school receiving less than three activities is considered a limited supports school. The differences in the number of pre-entry supports provided to schools were primarily a result of resource limitations. As the resources available to fund these activities varied from year to year, this affected the roll-out of all pre-entry services to all schools, with distance being a key factor. While most urban schools were not limited in the amount of supports they received, there are some rural schools which are classified as limited and other rural schools classified as non-limited support schools. Thus in order to examine whether varying the level of pre-entry supports affect student performance at university, while controlling for distance from the university, we re-estimate the results for students who attended limited pre-entry support schools and full pre-entry support schools.<sup>40</sup>

The results presented in Table 2.9 show that students who attended the full pre-entry support schools had better first year outcomes than students who attended the limited

<sup>40</sup> Note that information on the pre-entry activities in schools linked to other universities is not available. Thus this analysis is restricted to the UCD linked schools.

support schools. Attending a full pre-entry support school increases the probability of achieving honours by 16% and reduces the probability of passing or failing by 10% and 6% respectively. However, a Wald test reveals that the coefficients for the limited support schools and the full supports schools are not statistically different from each other ( $\chi^2=0.04$ ,  $p=0.853$ ). In addition, there was no statistical differences on the impact of the program on final year outcomes for the students who attended the limited pre-entry support schools and the full pre-entry support schools ( $\chi^2=1.11$ ,  $p=0.292$ ). Overall, this indicates that there is little evidence that students from the high support schools are systematically better than the students from the limited support schools, suggesting that the pre-entry activities are having a minimal effect on the pre-entry academic performance of the students and that the main results are not driven by selection bias.

## 2.5 Conclusion

This study examines the effectiveness of a multidimensional access program designed to increase the number of low socio-economic status students attending an Irish university and to improve the academic performance of such students within the university. Arising from the very low university participation rates of low SES students, the majority of Irish universities operate access programs which provide a combination of social, academic, and financial supports to participating students. Yet despite the prevalence of such access programs in Ireland, none have been evaluated to date. While there is some evidence that multidimensional programs can be effective (e.g. Angrist et al. 2009; Brock and Richburg Hayes, 2006), there is a dearth of research on this topic, particularly outside of North America.

A clear advantage of this study is that it includes multiple outcomes which go well beyond initial enrolment, which is the focus of much of this literature. The results indicate that the access program led to significant improvements in the academic performance of students, and unlike many previous programs, these effects persisted throughout their time in college. Indeed, a distinctive characteristic of this program is that it continues the academic, social, and financial support beyond first year. The program has significant positive benefits on a range of first and final year outcomes including exam performance and retention rates. Such positive effects are found for students whose university entrance grades met the normal entry criteria (Merit Students), and also for those (Discount Students) who were admitted with grades below the normal entry threshold for their course. It is particularly significant that this program reduced the probability of dropping out of university as withdrawal from a degree, in addition to the financial implications, may also

have long lasting self-esteem and stigmatizing effects particularly if the student has miscalculated their relative ability to finish the program. There are additional financial implications from failing exams, as during the time period under analysis, such students incurred full fees to repeat the year.

The program improves the first year exam performance of both Discount and Merit treated students, in that it increases the probability of achieving an honours degree and reduces the probability of either failing or only just passing. In addition, the AP improves the final degree classification. That there are no gender effects is contrary to much of this literature (e.g. Angrist et al. 2009), and the wider intervention literature (Anderson, 2008; Schweinhart et al. 2005; Clampet-Lundquist et al. 2006).

The program also has positive effects on the students' final year outcomes. It increases the graduation rate by ~5%. This effect is significant and represents the cumulative effect of the program in reducing drop-out at each stage of university life. This effect is in line with the graduation effects of about 3-6 percentage points found in financial aid based scholarship programs (e.g. Dynarski, 2008, Scott-Clayton, 2009). Given the high private returns to completing a university degree (e.g. Jaeger and Page, 1996; Callan and Harmon, 1999) this implies a significant financial benefit to access students who make it to graduation. The size of the graduation effects hold for both females and males, which differs somewhat to the literature, which typically finds larger effects for females in terms of course completion (e.g. Dynarski, 2008; Garibaldi et al. 2007). Increasing the graduation rates not only generates long term benefits for the students through increased employment opportunities and earnings, it also generates benefits to the university in terms of improving its reputation and maximising its resources. While the program helps the students to make it

to graduation, it has no impact on the probability that they will graduate on time. This contrasts with Scott-Clayton (2009) which finds that an incentives based program reduces time to completion. Graduating late will have explicit costs of studying for at least an extra year. In Ireland, there is an extra disincentive to not delay graduation as tuition fees, which are normally waived for Irish students, are payable for repeated years of a degree course. There are also implicit costs of forgone graduate earnings.

Given that the program has a positive impact overall on Discount students who received preferential entry treatment, affirmative action or positive discrimination does not appear to compromise academic standards. This suggests that grade concessions offers to low SES students may be an effective means of reducing educational inequality. Note that the positive discrimination made by the AP occurs on a relatively small scale both in terms of the absolute number of students admitted and the level of grade remission that an individual student receives. Nonetheless this study suggests that access program based on socio-economic status can be effective. Thus, calls for the US affirmative action programs to move away from the ethnicity based eligibility criteria to a socio-economic based criteria may prove effective.

It is important to recognise that these results may overstate the post entry effect of the AP if the program's pre-entry activities in high school raise the academic quality of the treatment group. As this study only observes students within the university, and not the initial pool of applicants, we cannot directly measure the extent of this problem. However, a comparison of the outcomes of students who attending high schools which received a high level of pre-entry activities, do not systemically differ from students who attended high schools which received fewer pre-entry activities. This suggests that the pre-entry activities

had a minimal effect on the pre-entry academic performance of the students and that the main results are mainly driven by the post entry intervention.

This study also finds that a high level of financial aid may not be the main contributor to university success as variation in aid over time does not adversely impact on student performance. However there are limits as to how much one can extrapolate from this result. Firstly, the sample size used for this analysis is quite low as only AP students can be included; therefore the estimated results may not be precise. Secondly, based on the data available, it is not possible to speculate with any degree of confidence if an increase by more than around €900 (about \$1,100 in 2010 prices) would have had any effect. Nor is it possible to estimate if a reduction in the value of the financial package below an amount of around €5,400 (about \$6,630) would have any effect on average student performances. However in reducing the amount of financial aid to students, consideration should be given to the possible effects of such a reduction on student employment as students may enter part-time employment to offset the reduction in financial aid (although there is currently no consensus in the literature on the effects of student employment on academic outcomes). Yet a recent paper (Denny, 2010), which found that the abolition of university fees in Ireland in 1996 had little impact on student progression to university, suggests that students in Ireland do not face credit constraints. Thus, as found here, reducing the value of financial aid does not appear to effect university performance. Unlike many US programs, financial aid in this AP is not conditional on achieving a particular grade, except where students need to repeat an academic year in which case they do not receive the aid during that year. See for example, Scott Clayton (2009), Dynarski (2008) or Angrist et al. (2009), for fairly mixed evidence on the effectiveness of such conditional aid.

While the design of the evaluation precludes an analysis of the relative effects of the different components of the program, this study has some general implications for the AP in regards continuing to support both Merit and Discount students and potentially reducing the size of financial aid. The access program discussed here started by treating students during high school, yet in recent years they have begun working with primary (elementary) schools. This is line with evidence that interventions which begin earlier in the lifecycle have the highest pay-off (see Carneiro and Heckman, 2003 for example). A strong argument can be made that society should invest in education to ensure that children's' education is not constrained by their parents' socio-economic status. While there is more than one cause of educational inequality, and hence there is no single global solution, the multidimensional intervention studied here is one useful tool available to policymakers that can help address this issue.



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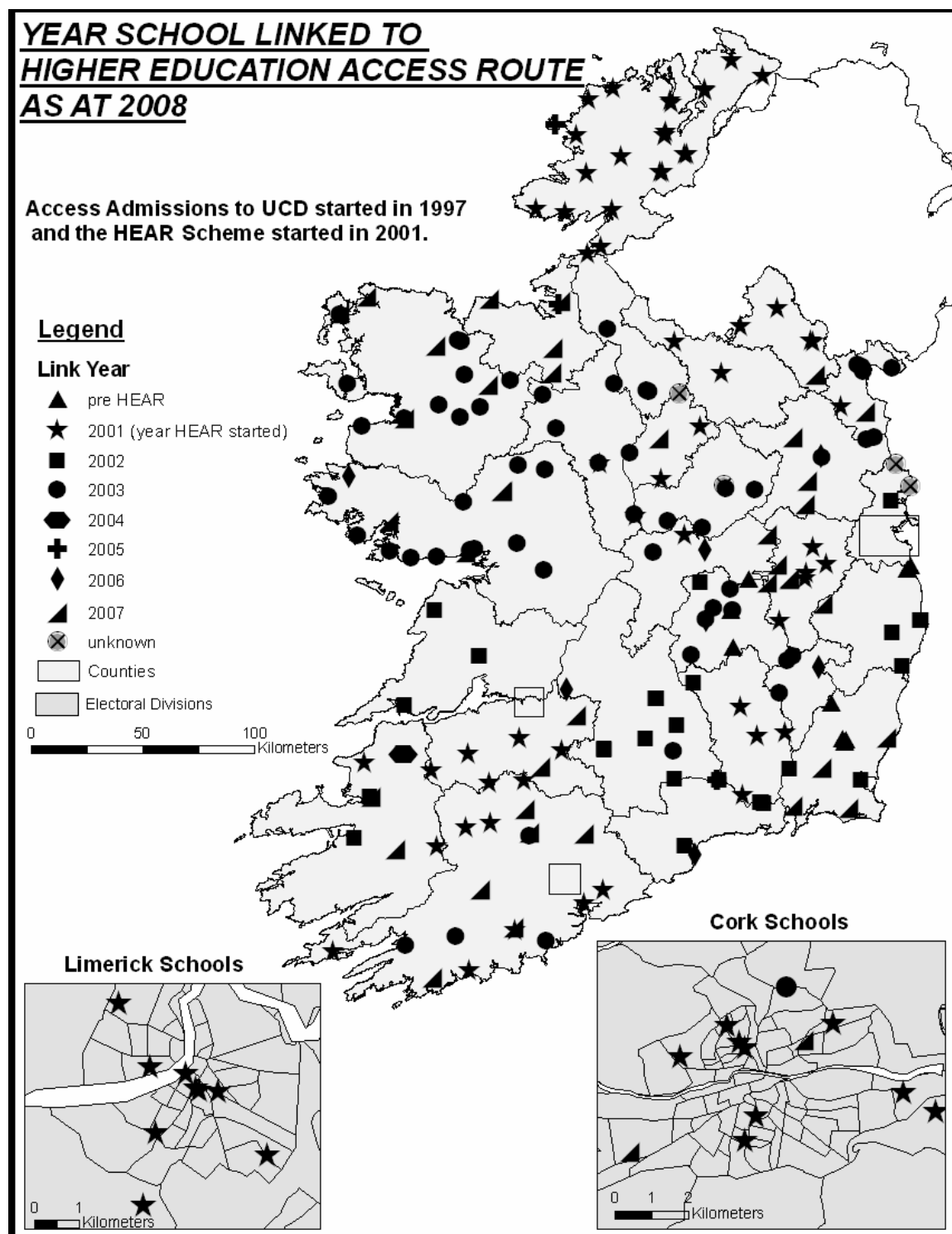
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## **Figures for Chapter Two**

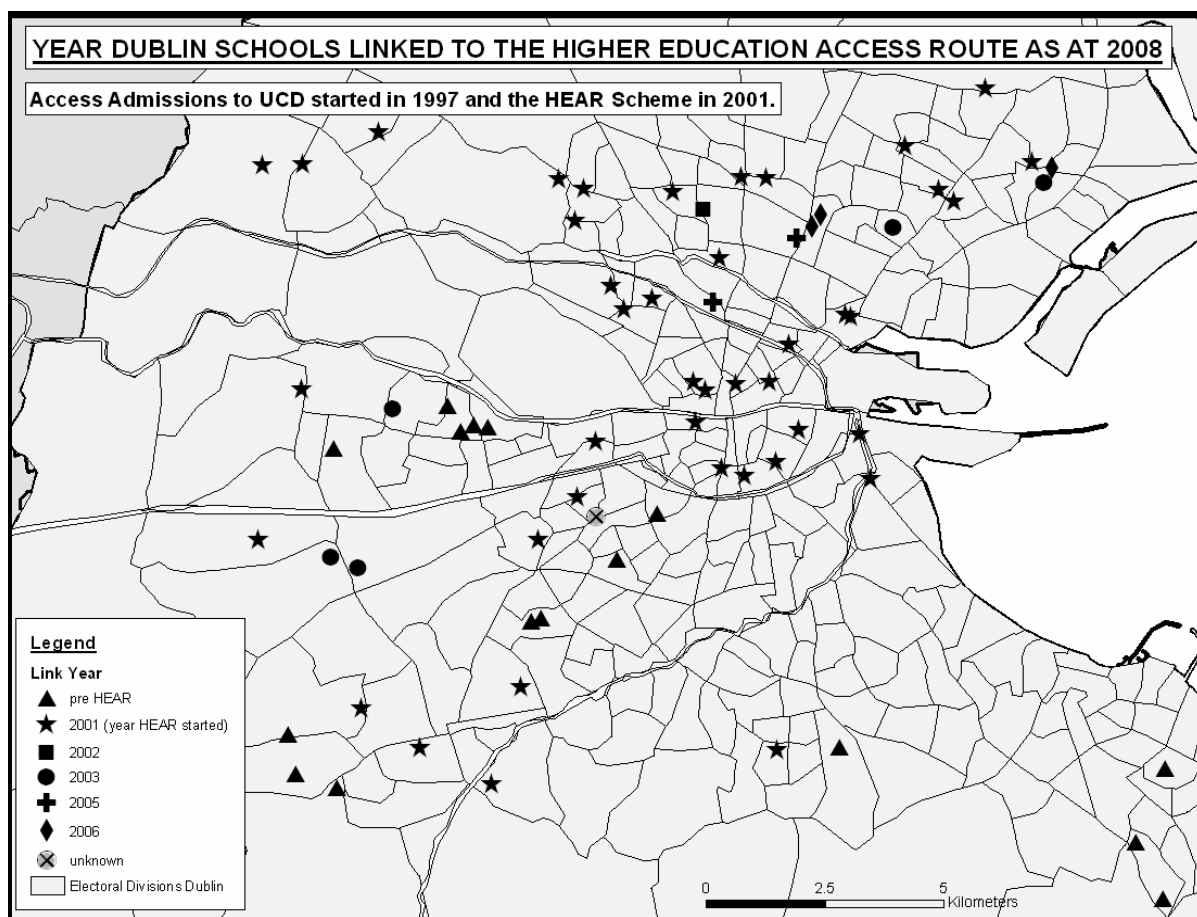
Figure 2.1 Program Expansion by Geographical Location in Ireland



Note: pre Hear refers to before 2001.

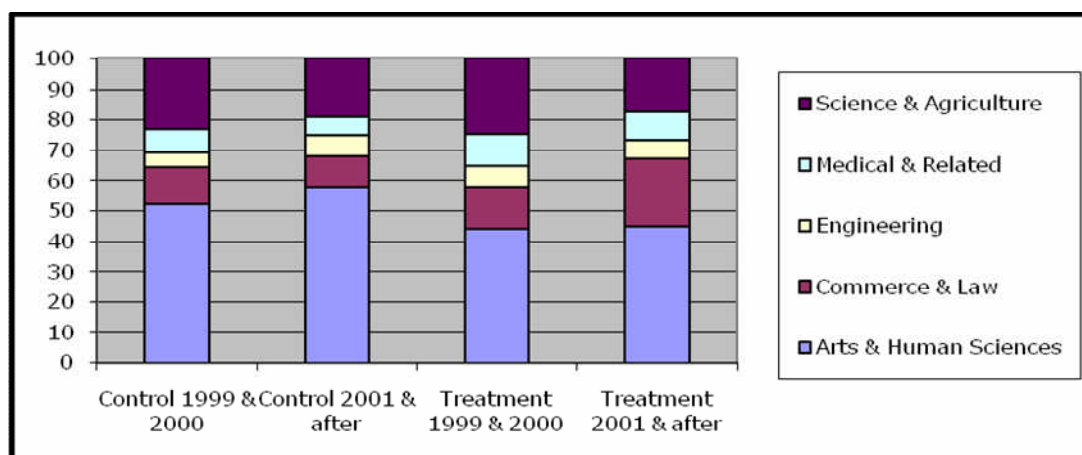


**Figure 2.2 Program Expansion by Geographical Location in Dublin**

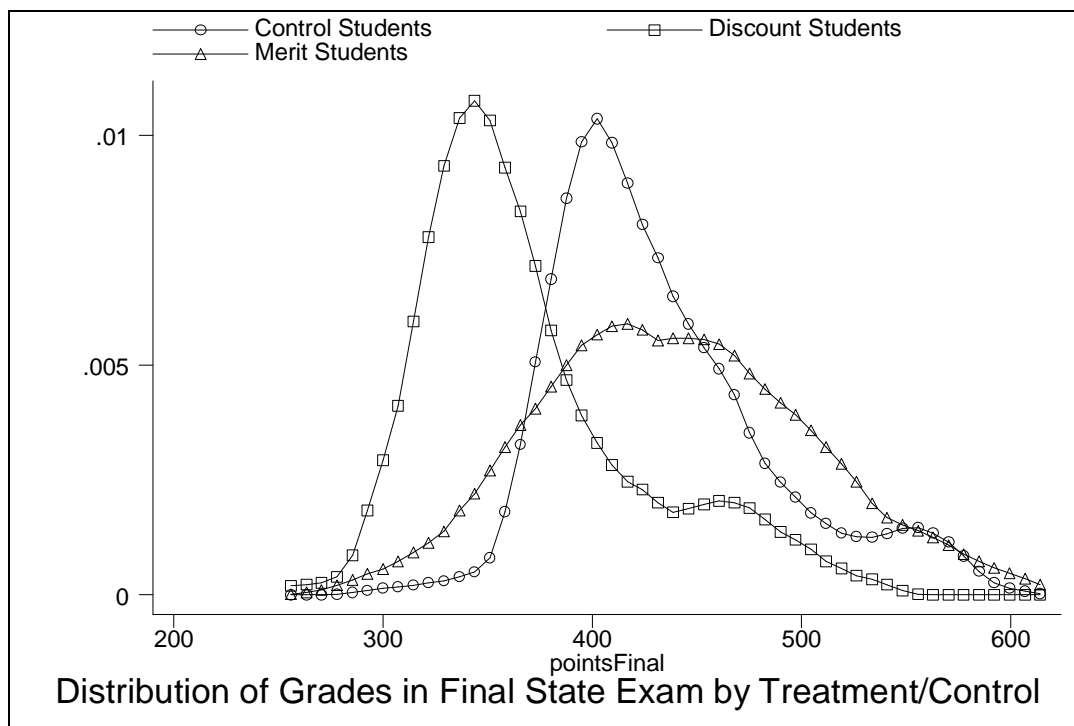


**Note:** pre Hear refers to before 2001.

**Figure 2.3 University Faculty by Treatment and Control Groups Before and After the 2001 Reform**



**Figure 2.4 Distribution of High School Grades in Final State Exam by Treatment and Control Groups**



## Tables for Chapter Two

**Table 2.1 Program Expansion and Commuting Distance from High Schools to University**

Year High School Became Linked to the AP	Not Within Commuting Distance to UCD	Within Commuting Distance to UCD
1999	5	14
2000	2	2
2001	34	30
2002	11	4
2003	35	11
Post 2003	10	11

**Note:** No. of high schools which joined the AP over time displayed by whether they are within commuting distance to UCD or not.

**Table 2.2 Labour Market Characteristics in Access Program School Localities by Year of Linkage**

Year of linkage to AP	Number of schools entering program	Proportion in locality unemployed in 1996	Proportion in locality leaving education before age 18 in 1996	95% CI Lower Bound	95% CI Upper Bound
1999 or before	21	.11 (.01)	.11 (.02)	.09	.13
2000	6	.06 (.01)	.14 (.03)	.04	.08
2001	93	.09 (.00)	.13 (.01)	.08	.10
2002	23	.09 (.01)	.11 (.01)	.08	.10
2003	58	.07 (.00)	.13 (.01)	.07	.08
2004 or later	40	.08 (.00)	.12 (.01)	.07	.09
National average		.07 (.00)	.18 (.01)	.06	.07

**Note:** Mean, standard deviation (in parentheses) and the lower and upper bound of the 95% confidence interval reported. The figures represent the average labour market conditions, as reported in the 1996 Census at the electoral division level, in the locality of the schools which joined the AP between 1999 and 2005. The average size of an ED is 20.4 sq km. The number of observations (at school level) are weighted by number of students from school in final sample. No school has left the access program having been chosen to join it.

**Table 2.3 Average High School Grades in Final State Exam for Treatment and Control Groups by Year of Linkage**

Year of linkage	Treatment	Control	T-test of difference
1999	377 (49)	380 (22)	.30
2000	411 (64)	437 (50)	1.30
2001	397 (59)	426 (47)	2.79***
2002	406 (63)	424 (52)	.59
2003 or later	425 (75)	433 (37)	.20

**Note:** Mean, standard deviation (in parentheses) reported. The average university entrance exam grades are based on final school exams consisting of 6 exams worth 100 points each, for a maximum score of 600 points.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

**Table 2.4 Student Characteristics**

Student Characteristics	General Student Population	Control group Students	Discount AP students	Merit AP students	Diff between Control & Discount	Diff between Control & Merit
	%	%	%	%		
Male	45.98	30.35	37.21	31.71	$\chi^2=1.39$	$\chi^2=.05$
In receipt of means tested state grant	16.94	100	100	100	$\chi^2=.0$	$\chi^2=.0$
Father's socio-economic group						
Higher	70.2	0.00	0.00	0.00		
Non-Manual	21.46	44.75	57.69	48.65	$\chi^2=2.91^*$	$\chi^2=0.20$
Manual	8.33	55.25	42.31	51.35		
Grade in final state exam						
300-350	3.12	1.56	48.24	6.10		
355-400	21.38	29.96	30.59	25.61		
405-450	27.75	40.47	8.24	28.05	$\chi^2=133.81^{***}$	$\chi^2=11.35^{**}$
455-500	23.06	16.73	10.59	26.83		
505-550	15.65	7.00	2.35	8.54		
556-600	9.05	4.28	0.00	4.88		

---

Year of entry to university					
1999	19.97	24.90	11.63	17.07	
2000	20.22	21.01	19.77	13.41	
2001	19.96	24.51	15.12	13.41	$\chi^2=23.02^{***}$ $\chi^2=19.43^{***}$
2002	20.43	19.46	25.58	39.02	
2003	19.42	10.12	27.91	17.07	
Sample size	13,478	257	86	82	

---

**Note:** Discount students are those who entered the university with reduced entry grades. Merit students are those who entered the university without reduced entry grades. The Control group include financially eligible students (i.e. grant holders), whose parents are not professionals or employers and who attended schools that subsequently became linked to the AP.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .



**Table 2. 5 First and Final Year Student Outcome Variables**

Outcome Variables	General Student Population	Control group Students	Discount AP students	Merit AP students	Diff between Control & Discount	Diff between Control & Merit
First Year outcomes:	%	%	%	%		
Honours	46.89	34.63	20.93	48.78		
Pass	41.46	52.53	61.63	42.68	$\chi^2=5.45^*$	$\chi^2=5.83^*$
Fail/Drop-out	11.65	12.84	17.44	8.54		
Final Degree outcomes:						
Honours	62.94	66.67	53.49	73.17		
Pass	14.81	16.08	23.26	15.85	$\chi^2=4.85^*$	$\chi^2=1.94$
Fail/Drop-out	22.26	17.25	23.26	10.98		
Sample size	13,478	257	86	82		

**Note:** Discount students are those who entered the university with reduced entry grades. Merit students are those who entered the university without reduced entry grades. The Control group include financially eligible students (i.e. grant holders), whose parents are not professionals or employers and who attended schools that subsequently became linked to the AP.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

**Table 2.6 Base Results: Impact of the Access Program on First and Final Year Outcomes**

	All	Discount	Merit
First year outcomes	(1)	(2)	
Honours	.122** (.054)	.130* (.071)	.121* (.063)
Pass	-.073** (.036)	-.083 (.053)	-.077* (.046)
Fail/Dropped out	-.049** (.019)	-.047** (.020)	-.044** (.018)
Pseudo R <sup>2</sup>	.163		.163
Sample size	425		425
Final degree outcomes	(3)	(4)	
Honours	.083* (.048)	.066 (.061)	.096* (.052)
Pass	-.033* (.020)	-.027 (.026)	-.040* (.024)
Fail/Dropped out	-.050* (.029)	-.039 (.035)	-.056* (.029)
Pseudo R <sup>2</sup>	.182		.098
Sample size	425		425

**Note:** All 4 models are ordered probit. Marginal effects and clustered standard errors (in parenthesis) reported.

The treatment effect is participation in the access program. Discount students are those who entered the

university with reduced entry grades. Merit students are those who entered the university without reduced entry grades. The Control group include financially eligible students (i.e. grant holders), whose parents are not professionals or employers and who attended schools that subsequently became linked to the AP. All models include: year of university entry, gender, number of points attained in final state exams, distance from the high school to the university in km, and local unemployment rates and education levels in the locality of the high school.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

**Table 2.7 Robustness and Sensitivity Results: Impact of the Access Program on First and Final Year Outcomes**

	Ability effects (low v high)		Gender effects (male v female)		Commuting distance effects (commuting v non commuting)		Fixed effects model
	Main Effect of AP	Interaction of AP & low ability students	Main Effect of AP	Interaction of AP & male students	Main Effect of AP	Interaction of AP & commuting distance	
First year outcomes		(1)		(3)		(5)	(7)
Honours	.107 (.078)	.030 (.100)	.102* (.054)	.065 (.098)	.133** (.067)	-.030 (.085)	.098 (.086)
Pass	-.064 (.050)	-.018 (.060)	-.060* (.035)	-.040 (.066)	-.080* (.044)	.017 (.046)	-.060 (.056)
Fail/Dropped out	-.044 (.029)	-.012 (.039)	-.041** (.021)	-.025 (.033)	-.054** (.024)	.013 (.040)	-.038 (.031)
Pseudo R <sup>2</sup>		.163		.163		.164	.257
Sample size		425		425		425	267

Final degree outcomes	(2)		(4)		(6)		(8)
Honours	.108	-.034	.099*	-.053	.102*	-.044	.108
	(.071)	(.108)	(.060)	(.101)	(.061)	(.106)	(.085)
Pass	-.043	.013	-.039	.020	-.041	.016	-.025
	(.030)	(.040)	(.025)	(.035)	(.026)	(.039)	(.020)
Fail/Dropped out	-.065	.021	-.059*	.033	-.061*	.027	-.083
	(.042)	(.068)	(.035)	(.066)	(.036)	(.068)	(.065)
Pseudo R <sup>2</sup>	.102		.098		.098		.175
Sample size	425		425		425		280

**Note:** All 8 models are ordered probit. Marginal effects and clustered standard errors (in parenthesis) reported. The first three columns report interactions for the students' ability using points received in final state exam, gender, and whether the student's high school is within commuting distance to the university or not. Both the main effect of the AP and the interaction effect are reported. The treatment effect is participation in the access program. The Control group include financially eligible students (i.e. grant holders), whose parents are not professionals or employers and who attended schools that subsequently became linked to the AP. All models apart from the fixed effect model include: year of university entry, gender, number of points attained in final state exams, distance from the high school to the university in

km, and local unemployment rates and education levels in the locality of the high school. The fixed effect model includes: high school dummies, year of entry, and number of points attained in final state exams.

\*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

**Table 2.8 Impact of Variation in Financial Aid on First Year Outcomes**

First year outcomes	Effect of financial aid package	
	Main Effect of AP	Interaction of AP & high aid package
Honours	.107 (.070)	.029 (.087)
Pass	-.063 (.046)	-.017 (.052)
Fail/Dropped out	-.043* (.025)	-.012 (.035)
Pseudo R <sup>2</sup>		.163
Sample size		425

**Note:** The model is ordered probit. Marginal effects and clustered standard errors (in parenthesis) reported. The model includes an interaction indicating whether the student received a high financial aid package or a low financial aid package. The Control group include financially eligible students (i.e. grant holders), whose parents are not professionals or employers and who attended schools that subsequently became linked to the AP. The model includes: year of university entry, gender, number of points attained in final state exams, distance from the high school to the university in km, and local unemployment rates and education levels in the locality of the high school.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

**Table 2.9 Impact of Access Program on Student Performance for Students from Limited and Full Pre-entry Support Schools**

	Effect of pre-entry AP supports	
	Limited pre-entry supports	Full pre-entry supports
<hr/> First year outcomes		
Honours	.204	.162**
	(.183)	(.078)
Pass	-.142	-.100*
	(.151)	(.057)
Fail/Dropped out	-.061*	-.062***
	(.034)	(.024)
Pseudo R <sup>2</sup>		.166
Sample size		358
<hr/> Final degree outcomes		
Honours	-.060	.077
	(.128)	(.060)
Pass	.020	-.030
	(.039)	(.025)
Fail/Dropped out	.040	-.047
	(.089)	(.035)
Pseudo R <sup>2</sup>		.091
Sample size		358



**Note:** All 2 models are ordered probit. Marginal effects and clustered standard errors (in parenthesis) reported. The treatment effect is participation in the access program. The Control group include financially eligible students (i.e. grant holders), whose parents are not professionals or employers and who attended schools that subsequently became linked to the AP. All models apart from the fixed effect model include: year of university entry, gender, number of points attained in final state exams, distance from the high school to the university in km, and local unemployment rates and education levels in the locality of the high school. The fixed effect model includes: high school dummies, year of entry, and number of points attained in university entry exams.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

## Appendix for Chapter Two

**Table 2A1 Comparison of British/Irish and North American Grading Systems**

US: GPA	British/Irish: Award
Greater than or equal to 3.68	First Class Honours
From 3.08 to 3.67 inclusive	Second Class Honours, Grade 1
From 2.48 to 3.07 inclusive	Second Class Honours, Grade 2
From 2.00 to 2.47 inclusive	Pass

# **Chapter Three: Piloting the Nation.**

## **What Do We Learn From Pilot Interventions?**

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“In many ways the social experiment method is the most convincing method of evaluation” Blundell and Costa Dias, 2002, p92.

### **3.1 Introduction**

Experimental evidence has become the “gold standard” of policy evaluation. It is becoming increasingly common to conduct experimental studies where one group is randomly allocated a treatment while another group is used as a control. Prominent examples of such studies include the National Supported Work Demonstration (Lalonde, 1986), the Canadian Self Sufficiency Project (Card and Robins, 1998), the STAR (Krueger and Whitmore, 2001) or the Mexican Progresa (Behrman, Sengupta and Todd, 2005). In the absence of random allocation, a pilot scheme may be introduced in part of a country, to test the implications of a policy on a small scale, typically by matching treated and control groups, as for example in the UK New Deal for Young People (Blundell, Costa Dias, Meghir and Van Reenen, 2001). The results of such experiments and pilots are then analysed before implementing the policy on a wider basis. The UK Civil Service (Jowell, 2003) and the US Department of Education (Coalition for Evidence Based Policy, 2003) for example, place a premium on the results of experimental “pilot” studies with significant weight also given to the results of quasi-experimental “pilots”. The

estimates of such an experimental approach are believed to have a high degree of internal validity<sup>41</sup>.

However the external validity of pilot results is not often questioned even though there are several reasons why the effects of the pilot scheme and a later national implementation of a scheme may differ. This usually has to do with differences in information, costs and general equilibrium effects. Firstly, there may be a time-lag of several years between the implementation of the pilot scheme and the extension of the policy to the full population. The change in the macroeconomic conditions may then alter the behaviours of the individuals subject to the policy. Secondly, the characteristics of the areas/groups treated by the pilot scheme may differ from those of the areas/groups to which the policy will be later extended. This is the case for example if the pilot is implemented on an area/group that has higher risk or worse characteristics than the area/group at which the policy is later extended to. Third, the parameters of the pilot and national roll-out of the policy may differ; i.e. a relatively generous pilot scheme may be too costly to implement on a wider basis<sup>42</sup>. Fourth, there may be general equilibrium effects that do not materialise in a small scale pilot but alter the behaviour of all agents, treated or not, under the national implementation.

In this chapter we estimate the effects of the national extension of the Education Maintenance Allowance (EMA), a conditional cash transfer scheme aimed at increasing

<sup>41</sup> For a broad and non-technical discussion of the threats to internal validity in social experiments see Government Social Research Unit (2003). These include mobility post allocation to the treatment or control group, contamination of the control group or Hawthorne effects where participants and/or implementers behave differently as they know they are in an experiment.

<sup>42</sup> Although the per recipient administration costs may fall under economies of scale.

participation in post-compulsory education. Following positive evaluations of experimental pilot schemes which started in 1999 (Dearden, Emmerson, Frayne and Meghir, 2009), the policy was extended to the whole of the UK<sup>43</sup> in 2004. Using comparable dataset and methods, we compare the pilot and national roll-out estimates of the effect of EMA on post-compulsory participation.

The EMA was introduced nationally in September 2004 to alleviate the relatively low rate of post-compulsory education in the UK. As such, EMA is a conditional cash transfer scheme, not unlike Progresá in Mexico for example, which provides 16 to 18 year olds staying in full-time education with up to £30 per week. The allowance is means tested and can be claimed for up to two years of post-compulsory education<sup>44</sup>. Starting in the academic year 1999/2000 EMA was piloted in 15 Local Education Authorities (LEA) in England<sup>45</sup> with an additional 41 LEAs joining the pilot scheme the following year<sup>46</sup>. These pilot areas could be matched to similar local authorities (i.e. in terms of demographics and educational attainment) to estimate the effect of the allowance. Using this design, Dearden, Emmerson, Frayne and Meghir (2009) estimate an increase in post-compulsory education of 4.5 percentage points in the piloted areas. Based on the initial findings of the experiments, it was decided to keep the EMA in the pilot areas and expand

<sup>43</sup> EMA Schemes were launched across England, Scotland, Wales and Northern Ireland in August 2004. However the scope of this chapter is restricted to England as all the pilots were in England and because there are certain (although sometimes slight) institutional differences in the education systems of the constituent countries of the UK.

<sup>44</sup> Most further education courses are two years in length but there is provision for EMA funding for a third year in some circumstances.

<sup>45</sup> Among these 15 original pilot areas there were variations in the payment levels of EMA.

<sup>46</sup> Four of these LEAs used “Transport EMA’s” designed to fund students’ travel needs. These are excluded from the analysis in this chapter.

it to the rest of England for the academic year 2004/05. EMA now reaches more than 500,000 youths at a cost of £500 million per year.

The external validity of the results of the EMA pilot scheme potentially suffers from some of the shortcomings highlighted above. The EMA pilot areas were not nationally representative and had initially lower rates of participation in post-compulsory education than the rest of the country (see Figure 3.1). Moreover, there was a five year delay between the piloting and the national implementation of EMA which became less generous in real terms. The nominal amount of maintenance has remained identical since the original pilots in 1999 despite the retail price index increasing by 13% between 1999 and 2004. Similarly the household income eligibility thresholds were altered so that fewer families could claim EMA but more became eligible for the full amount of maintenance. Lastly the pilot study cannot capture general equilibrium effects that may occur once the scheme is introduced on a wider scale. For example the post-compulsory education system may experience capacity constraints when large numbers of students decide to stay on in education. The incentive effects of the EMA may have effects on national vocational training programmes and may cause changes in relative wages of school-leavers and apprentices. Due to crowding out and signalling, the EMA may also affect the education decisions on non-recipients.

### **3.2 Policy Context**

Participation in post-compulsory education increased substantially in England in the mid-eighties from 50% of 16 year olds continuing in education to about 70% by 1992 but it flattened at this level for the rest of the nineties (Figure 3.1) and growth thereafter was slow. By international standards, post-compulsory education is thought to be low; the percentage of 15-19 year olds in the UK engaged in full-time or part-time education is 69.7% which ranks third last amongst OECD countries and does not compare favourably to the 81.5% OECD average (OECD, 2008). Moreover, there is a large gap in participation by parental characteristics which has only narrowed slowly over time (Blanden, Gregg and Machin, 2005). Identifying the mechanism causing this gap is difficult. Family characteristics dominate short term financial constraints (Chevalier and Lanot, 2002) but Dearden, McGranahan and Sianesi (2004) find that about 8% of pupils face short term financial constraints and recommend that targeted policies be directed towards students at the age of 16 rather than in later years.

In response to these problems and following positive evaluations of the pilot schemes, the EMA was introduced throughout England (and indeed in the entire UK) in time for the 2004/05 academic year. The scheme provides 16 to 18 year olds a weekly cash allowance conditional on studying full-time. The allowance is means-tested on



parental income<sup>47</sup> – but not the teenager income, so that part-time work while studying is not discouraged. In England, the household income thresholds at which the allowance is tapered have changed over time and for the academic year of 2010/2011 set at £20,817, £25,521 and £30,810. The weekly payment levels to young people from families below these thresholds are £30, £20, £10 respectively. The money is directly credited to the pupil bank account and can be spent on any good. Additional bonus payments are also available twice a year conditional on achieving personal educational targets, to improve retention and attendance.

Before the national implementation of the scheme, EMA was piloted in about a third of local authorities in England starting with 15 LEAs volunteering in 1999 and another 41 joining the year after. Small differences existed in the implementation of the pilots to test the policy parameters of interest. The variations in the scheme included whether the young person or the mother received the EMA and whether the maximum allowance was £40 or £30 per week. Compared to the national extension, the piloted EMA had six to eight £5 bands rather than three £10 ones.

Note that there is a delay of 5 years between the evaluated pilot and the national roll-out. Moreover, the parameters of the EMA have changed between the pilot and the roll-out. EMA has become less generous, the maximum payment being £30 rather than £40 in some of the pilots, moreover, this maximum has not been indexed to inflation,

<sup>47</sup> The EMA is not calculated in the calculation of any other benefits the family may be eligible to. Additionally, each family whose child aged 16-18 is in full-time education is eligible for child benefit while low income family may also be able to claim child tax credit. So any EMA payment is fully captured by the household.

which over this period ran at 13%, so in real term the allowance is 13% less generous in the roll-out than in the pilot. The eligibility thresholds have also been altered. The top threshold has remained fixed in nominal terms, so that with inflation and a general increase in the income distribution, fewer families are eligible for EMA. However, more youth qualify for the maximum allowance since the lower threshold has been pushed up from £13,000 in 1999 to £19,630 in 2004. Figure 3.4 summarises the impact that the new parameters have had on the proportion of youth eligible.

### **3.3 Literature Review**

There is currently a vociferous debate regarding the methodology of empirical microeconomics. Much of the debate is in the context of development economics, however the arguments are, for the most part, not specific to that area.

One group of researchers (e.g. Imbens (2010), Angrist and Pischke (2010)) advocate an experimental approach to policy evaluation as the only means to achieving internal validity. However another group (e.g. Heckman (2010), Deaton (2010), Rodrik (2008), Ravallion, (2009)) have concerns that the experimental approach only identifies effects but does explain the mechanisms driving these effects and hence represents an atheoretic “black-box” approach to science. A consequence of this is that experimental studies may have low external validity. Another concern is that an over-reliance on experimental studies is resulting in the neglect of areas where experiments cannot be easily constructed.

Some researchers such as Heckman and Smith (1995) have concerns that the experiments themselves do not provide answers to questions that policy makers have – they only provide mean effects rather than the proportion of people who gained and lost as a result of the introduction of a policy. They also have issues with the actual practical implementation of experiments. For example, members of control groups may attempt to obtain the treatment or find some substitute elsewhere.

The aim of this paper is to look at one area relevant to the current debate: whether the effects of a pilot scheme, estimated using quasi-experimental or experimental methods, still hold once the scheme has been “rolled out” or “scaled up” on a wider or perhaps

even a national basis. Moffitt (2006) presents a taxonomy of “scale-up” effects. Firstly, information diffusion effects may cause changes in the characteristics and the numbers of people applying for or being affected by a policy. Secondly, the nature of the policy may change from the pilot stage to an expanded stage. The nature of the treatment may alter as administrators learn from past experience or with increased political interference. Lastly there may be general equilibrium effects. The policy may alter market prices. Heckman, Lochner and Taber (1998) simulate the effects of a tuition subsidy in a general equilibrium setting where the enrolment effects of such a policy cause skill prices (high school educated workers versus university educated workers) to change and where the funding of such a subsidy through taxation is taken into account. In their study, the effects of a policy are smaller once the general equilibrium effects are taken into account.

Duflo (2004) whilst advocating a quasi-experimental or preferably an experimental approach to policy making, recognises that the scaling up or rolling out of pilot schemes may involve many of the problems mentioned by Moffitt (2006). However she points out certain strategies available to program designers that may improve the likelihood that the effect of the policy at the pilot stage will be similar when implemented on a wider basis. For example, the pilot scheme should be implemented in several different settings. In the case of the EMA, the pilots were run (and matched with controls areas) in inner city urban LEAs, mixed urban-rural LEAs and in predominantly rural LEAs. Different variations of the pilot scheme should be run to identify which aspect of the scheme is having a larger effect than others. She argues that although running pilot schemes in different locations and running pilots with variations in treatment might be

expensive, it would be cheaper in the long run than running an ineffective policy nationally.

EMA is an example of a “conditional cash transfer” policy which provides a payment to certain groups of people contingent on their family income and contingent on certain behaviours being followed. Similar schemes operating in developing countries (e.g. Mexico, Brazil and Columbia) have received much attention in recent years (see Rawlings and Rubio (2005) for a review). These schemes target extremely poor families and the payments are made subject to the families investing in the human capital, both educational and health, of their children. Such schemes are rare in developed countries, however a recent exception is New York’s “Opportunity NYC – Family Rewards” program which rewarded low income families for engaging in activities related to child education and family health. Initial findings from Riccio, Dechausay, Greenberg, Miller, Rucks & Verma (2010) suggest that there some positive effects on academic credit accumulation and school attendance for certain subgroups

The Australian AUSTUDY scheme is the policy most similar to EMA. At around the time the scheme was launched, Australia was ranked relatively lowly amongst OECD countries in terms of participation in upper secondary education. Its ranking was usually very close to that of the UK’s depending on the measure used. AUSTUDY was launched in 1987 and covered the final two years of high school (as well as higher education). Like EMA, the payment was made to the child rather than the parent (although an EMA variant where the payment was made to the mother was piloted). All payments were means tested on parental income. Dearden and Heath (1996) found that AUSTUDY

increased post-compulsory schooling by 3-4% amongst poorer students and they recommended that the UK adopt a similar policy.

Dearden, Emmerson, Frayne and Meghir (2009) conducted the evaluation of the EMA pilot schemes by matching individuals in nine of the original treated areas with individuals in specific control LEAs, and estimated that the EMA increased participation by 4.5 percentage points for the eligible young people. Chowdry, Crawford and Emmerson (2008), also estimated the effect of the pilot EMA but using administrative data. This has the advantage of a much larger sample size but has the disadvantage of a very limited set of covariates which leads to a noisy measure of EMA eligibility. They found that EMA increased participation by between 5.5 to 7.3 percentage points for women and by between 2 and 5.5 percentage points for men (although some of the estimates were not significant for men) and an average treatment effect on the treated ranging from 0 to 3%. Chowdry, Crawford and Emmerson (2008) also highlight the heterogeneity of these results as the impact of EMA on participation is more than twice as large in the most deprived quintile than in the least deprived one.

### 3.4 Identification Method and Data

To estimate the effect of the pilot scheme on eligible students we estimate the following triple-difference model:

$$Y_{ijt} = a_0 + b_1 E_i + \sum_k b_{2k} LEA_k + b_3 T_t + b_4 X_{ijt} + c_1 E_i A_j + c_2 E_i T_t + c_3 A_j T_t + d_1 E_i A_j T_t + \varepsilon_{ijt} \quad (1)$$

For an individual  $i$ , living in LEA  $j$ , at period  $t$ ,  $Y_{ijt}$  is a dummy variable indicating whether the student is in full-time education in the spring of the academic year following her last year of compulsory education.  $E$  denotes the student' eligibility for EMA,  $T$  whether the observation is for a period when the pilot was in place, and  $A$  whether the LEA is conducting the EMA pilot or not. In this case the pilot areas are the “treatment group” and the Rest of England is the “control group”. Neither area had EMA when  $T=0$  but EMA had been introduced in LEA's from area  $A=1$  when  $T=1$ .  $X$  a vector of personal individual characteristics and  $\varepsilon$  a random term. This linear probability model is estimated by OLS using standard errors clustered at LEA level and robust to heteroskedasticity. The coefficient  $d_j$  is an estimate of the effect of the EMA pilot scheme on eligible students in the pilot areas, i.e. the average treatment effect on the treated group. Similarly, we can estimate the effect of the national roll-out of the EMA on eligible students. The estimated model is similar to (1) but  $A$  is now an indicator of being in an LEA implementing the roll-out, i.e. did not implement the pilot, and  $T$  is an indicator for the individuals being surveyed post 2004.  $d_j$  is now the effect of the introduction of the national programme on eligible people and the pilot areas are used as controls.

Figure 3.2 shows that initially there was a large gap in education attainment between the pilot areas and the Rest of England<sup>48</sup>. The trends in the proportion of students remaining in education in the mid-1990's are similar for both the pilot areas and the rest of the country. Then there is a sharp kink in the post compulsory participation levels in the pilot areas at the time of the introduction of the pilot programme. This story is consistent with existing estimates of the effect of the EMA. However following the national introduction of the scheme, there does not appear to be a significant increase in the proportion of students in the rest of the country staying on relative to the pilot areas where the scheme was always in operation. Instead the series seem to be converging.

As we can see in Figure 3.2 from around 2004 the proportion of young people staying on in full-time education rose in both the pilot areas and in the rest of England. In the former, the proportion rose by about 8 percentage points from 2002 to 2007 and in the latter, the proportion rose by about 6 percentage points. This is in spite of the EMA being introduced into the rest of England in 2004 having been already in the pilot areas for several years. What event could have happened which affected the pilot areas more than the rest of England when one would have expected the rise in participation to be greater in the rest of England than the pilot areas due to the EMA being rolled out? One possible explanation could have been the rise in youth unemployment taking place in the UK around the same time.

<sup>48</sup> The oversized data points represent the points for which usable YCS are available.



Figure 3.3 shows the rate of unemployment amongst 16-17 year olds over time in the UK. There is a rise in this rate from 2004 onwards. As the pilot areas were, on average, more economically deprived than the rest of England, it is plausible that this rise would have been greater in the pilot areas. With unemployment rates high, full-time education could be seen as a “safe haven” by those deciding on their options and hence the overall rise in participation in full-time education.

More formally we test whether the trends pre-pilot were the same. For this we rely on LEA level data on the proportions of 16 year olds in post-compulsory education. This data is available from 1994 but changes in the geography of some LEAs over time means that we rely on an unbalanced panel for this test<sup>49</sup>. We test whether pre-pilot similar linear trends existed between pilot LEA's and the rest of the country. Table 3.1 reports these OLS estimates. While there are differences in levels, the trends in post-compulsory education are similar between the two regions. Thus, the rest of the country could be used as a control for the pilot areas. The second part of Table 3.1, check for trend differences between the pilot and the rest of the country between the introduction of the pilot and the national roll-out. Again, we do not find any significant differences in linear trends between regions. However it should be noted that this analysis excludes London (both Inner and Outer) as it was not possible to disaggregate the LEAs within London into those that had participated in the pilot and those that did not.

<sup>49</sup> The results presented in Table 3.1 are not sensitive to the treatment of the LEAs whose definition changed over time. Excluding them from the analysis, keeping only observations after 1997 do not alter the conclusion of no significant difference in the trends between pilot and rest of the country.

To evaluate the effects of the pilot scheme and the national implementation of the EMA, we use a series of cross sections spanning the period of interest. The Youth Cohort Study of England and Wales is a recurrent longitudinal survey of young people beginning in the year after their last year of compulsory education. Since EMA was only piloted in England, we only keep England residents. For each cohort there are usually three annual sweeps of these surveys. For this study we only use the first sweep – the Spring after the student completed compulsory education - as there is a high attrition which is non-random and much greater amongst those who do not stay in full-time education. Information on post-compulsory education, labour market activity and some background demographics is collected.

To evaluate the effect of the pilot scheme we use YCS 9 and YCS 11<sup>50</sup>. Cohort 9 was surveyed in 1997/98 before EMA was implemented anywhere and constitute the pre-treatment period. Cohort 11 was surveyed in 2001/02 when EMA was piloted in about a third of the country. To estimate the effect of the national implementation of the EMA, we use Cohort 11 and Cohort 13 (surveyed in 2006/07)<sup>51</sup>. In this case the control group is those LEAs where EMA was first piloted and the treatment group is the rest of the country.

<sup>50</sup> Cohort 10 consists of those who could enter post compulsory education in the academic year 1999-2000. This is not used in this study as the EMA was only piloted in 15 areas that academic year and our estimates would rely on a very small sample.

<sup>51</sup> Cohort 12 could enter post-compulsory education in 2003/2004 and would have perhaps made a better pre-treatment group. However for Cohort 12, unlike Cohorts 11 & 13, questions relating to EMA were not asked until the second sweep. Given an attrition rate of 30% from sweep 1 to sweep 2, the sample size falls considerably, with those in education in sweep 1 proportionately less likely to drop from the sample.

Table 3.2 shows descriptive statistics relating to the backgrounds of the cohorts in question. This confirms Figure 3.2 and shows that EMA pilots LEAs are different from the rest of the country. In comparison to youths from LEAs not in the original pilot, youths in the pilot areas are more likely to live in households without one of their parents, where parents may not be working and where parents are less educated. Maybe due to this less favourable background youths in the original pilot areas are less likely to be in education in the year following their last year of compulsory education and also less likely to have scored well in the GCSE exams when compared to students from the rest of England.

In our analysis will examine effect of the EMA on young people eligible for the programme. The EMA eligibility rules are based on total family income. Unfortunately there is no parental income information in the YCS and we use the following procedure to determine EMA eligibility. First, EMA eligibility is assumed when no adult in the household is working. Second, when at least one parent is working and present in the household, a weekly labour income is estimated using the Labour Force Survey (LFS). In those cases, each parent's labour income is predicted by fitting that parent's characteristics (education, race, occupation, seniority at work and region of residence) to regression results from separate male and female earnings regressions estimated using contemporaneous LFS data. Families with self-employed parents are excluded from the analysis as it is not possible to infer their income using this method as the LFS regression is based on employees only. Table 3.2 shows how the final sample is arrived at.

For each cohort, youths are predicted to be eligible for EMA if their predicted family income is less than the year's threshold for claiming even the minimum amount of

EMA. Table 3.4 shows how our prediction of eligibility performs. The first row shows the proportion of young people predicted to be eligible for EMA in England for each cohort of YCS broken down by the pilot status. For the pre-pilot cohort we assume that the threshold of eligibility would have been those used in the pilot scheme but adjusted for inflation. The proportion of eligible people falls over time. This may in part be due to differences in the accuracy of our predictions across the different cohorts. However it should be noted that the threshold for the pilots was £30,000 and the threshold for the national scheme was £30810. Since the rise in the RPI over the period was 13%, the real value of the threshold fell. The proportion of eligible young people is higher in the pilot areas than the Rest of England as the pilot areas were chosen on the basis of having relatively higher levels of deprivation.

Table 3.4 shows that in YCS 13 (2006/2007) roughly half of the young people in YCS are predicted to be eligible for receipt of the EMA<sup>52</sup>. The proportion of students actually claiming in Cohort 13 is about 45%. This is slightly higher than the 40% yielded when one divides the number of recipients in 2007/2008 (546,472<sup>53</sup>) by the number of 16 and 17 year olds in England (roughly 1.3 million). The procedure for predicting eligibility appears to perform reasonably well, but about 7% of teenagers are predicted to be non-eligible but actually claiming EMA<sup>54</sup>.

<sup>52</sup> Other calculations we have carried out using FRS data gives a slightly higher estimate of around 55-60% of families with children being eligible.

<sup>53</sup> [http://www.lsc.go.uk/providers/statistics/learner/EMA\\_take\\_up.htm](http://www.lsc.go.uk/providers/statistics/learner/EMA_take_up.htm)

<sup>54</sup> Our estimates of the effect of EMA are somehow sensitive to how these people are coded (see result section).

There is also a small proportion of youths who are estimated to be eligible for EMA and are studying full-time yet are claiming not to be in receipt of the EMA. This could be caused by noise in the method of inferring eligibility, indeed a disproportionate number of these non-recipients come from families with both parents working, or error in reporting EMA status. This could also indicate that the take-up rate of EMA is not 100%, either because young people are not aware of the subsidy or because the costs, financial and not of applying are greater than the benefits, especially for youths only eligible to collect £10 per week.

Table 3.4 also shows some other anomalies. Firstly, 6% of students in those areas which were not original pilot areas are receiving the EMA in 2001, before it was introduced nationally. This could be because some students moved to pilot areas after completing their compulsory education or just indicate misreporting. Unfortunately we cannot resolve this problem, as our LEA identifier only relates to the LEA of the school where they spent their last year of compulsory schooling.

Table 3.5 replicates the descriptive statistics presented in Table 3.2 broken down by whether the student is eligible for the EMA or not. There are large differences in the characteristics of youth by eligibility status. EMA eligible youth are much less likely to be living with their fathers. In Cohort 13 only half of eligible youth have working fathers for non-eligible youth this proportion is above 90%. Their parents are also much less educated and they are more likely to be from a non-white ethnic background. For eligible youth we observe a large drop in the proportion of youth with less than 5 high grades GCSE over time but they still lag far behind non-eligible young people. Among the eligible youth, those from the pilot areas tend to have worse characteristics than those in

the rest of England while the differences between areas are less marked for the non-eligible youths. The characteristics of eligible young people have deteriorated over time which can be partly explained by the fall, in real terms, in the income threshold to receive EMA. Indeed, the last column shows the characteristics of eligible youths in Cohort 13 had the income threshold kept pace with inflation. In this case the eligible group in Cohort 13 would not have been as disadvantaged.

## 3.5 Results

We separately estimate the effect of the pilot scheme and the effect of the later introduction of the EMA to the entire country using the same methodology and equivalent datasets. Our estimates are of the average treatment effect on those eligible to receive EMA. The estimate is thus the effect of the intention to treat as some eligible individuals do not claim EMA. We do not differentiate between those eligible for £30, £20 and £10 as our method of predicting eligibility is noisy<sup>55</sup>. According to our YCS sample, around 80% of all EMA recipients receive the full £30.

### 3.5.1 Effect of the Pilot Scheme

One may ask how sensitive our results are to our method of inferring eligibility, as such Table 3.6 report the estimates when different assumptions regarding eligibility are made with eligibility being defined as described previously. For each different sample, we report the estimates for all youths and also separately by gender. The outcome of interest is whether the teenager is observed studying in full-time post-compulsory education. In Column A, the estimates are imprecise and none are found to be significant.

As shown in Table 3.4, there is some inconsistencies between the predicted EMA eligibility and the reporting of EMA receipt in the YCS. The remaining columns of Table 3.4 show the sensitivity of the results to the treatment of these inconsistencies. In Column B we have recoded all EMA recipients as being eligible regardless of their prediction (i.e.

<sup>55</sup> While our method for predicting EMA eligibility performs reasonably well, predicting the level of EMA payment is very noisy. For those receiving £20 and £10 in YCS 13, 70% are mispredicted to receive £30.

as eligible, as ineligible or undetermined due to missing information of parental characteristics). The pilot scheme estimates become larger and more precisely estimated. The pilot is estimated to have increased post-compulsory education by 10 percentage points, with large gender differences. The estimate are 4 percentage points but insignificant for boys but as large as 16 percentage points for girls.

Table 3A3 shows descriptive statistics for three groups of people. Those who were in receipt of EMA but were predicted, under the imputation process, to be ineligible numbered 474 observations. Members of this group are more likely to come from families where the father is present, where the parents are working and where the parents are better educated than those who had been predicted to be eligible for EMA as the prediction procedure used these data to infer eligibility.

Table 3A3 also includes those who were in receipt of EMA but where eligibility could not be imputed (1533 observations). Members of this group disproportionately come from families where the father is self employed. This was to be expected as the imputation procedure was only carried out for families with fathers who are employees. However in other respects such as parental education and prior academic achievements they are more similar to the eligible people than the ineligible people although they are more advantaged than the latter.

In the last set of estimates presented in Table 3.6, we recode as missing any recipients of EMA who have been predicted as ineligible. The estimates are halved compared to those obtained when all recipients were recoded as eligible. The only significant result that remains is for girls who are found to increase their participation by 10 percentage points.



In our favourite model, where all recipients are recoded as eligible, the estimates are larger than those of Dearden, Emmerson, Frayne and Meghir (2009) who found that the full-time education participation rates increased on average by 4.5 percentage points. It should be point that they use only 9 of the original pilot areas matched to 9 control areas. The estimates are also larger, although not by as much, than the estimates of Chowdry, Dearden and Emmerson (2008) who use administrative data to estimate the effect of the pilot scheme in the pilot areas which joined in 1999 and 2000.

### **3.5.2 Implementation of National Scheme**

Table 3.7 replicates the analysis for the national roll-out of EMA for the different samples specified. In the first sample, the estimates are again found to be imprecise. Recoding all recipients to eligible improves the precision dramatically and the roll-out is estimated to have led to an 8 percentage point increase in participation. Again the effect is larger for girls (10pp) than for boys (6pp). The effect for girls is statistically significant. Dropping the ineligible claimants halved the estimates, which were no longer significant.

Finally, with our favourite samples, we assess the heterogeneity of the effect with respect to the students' academic ability. Academic ability may affect the impact of EMA on the decisions of students to invest in post-compulsory schooling. One could expect an inverted U-shape effect where students at the top and bottom of the distribution do not revise their choice, and where the effect of EMA is the largest for marginal students whose academic abilities make them indifferent between dropping out or staying on. In Table 3.8, we report our results for two different specifications. We use a cut off of five GCSE grade A\*-C to split the sample as this is the usual requirement to carry on.

We find that the positive effects of the pilot scheme are driven by higher achieving students. We do not find statistically significant effects for the national-rolling out.

Over all samples, we find that the estimated effects of the roll-out are smaller than those obtained from the pilot. However, the estimates are never significantly different between the pilot and the roll-out, partly due to the lack of precisions of some of the estimates.

### **3.6 Discussion of Results**

The evidence of the national implementation having an effect on participation appears somewhat weaker than those suggested by the pilot scheme. Indeed, Figure 3.2 even suggests no effect of the roll-out, while individual level analysis in our favourite sample, shows that the effect is two percentage points lower in the roll-out compared to the pilot (but not statistically significant).

There are several reasons that could explain these weaker effects. First, the original pilot areas were disadvantaged local education authorities with lower rates of young people staying in full-time education. These local authorities may have experienced a different trend in participations. The differences-in-differences technique allows for different initial levels between the treatment and control areas. However it does assume that in the absence of the treatment, there would have been common trend over time in the average outcome in the treatment and control areas. Perhaps this is an unrealistic assumption and that differing trends over time would have applied to less well-off areas and better-off areas due to other government policies.

Our estimates attempt to mitigate against this by relying on a differences-in-differences-in-differences strategy which uses non-EMA eligible students from the pilot areas and the rest of England who would be unlikely to be affected by government interventions (i.e. they would have continued in full-time education regardless). Rather than assuming common trends on average in the pilot areas and the rest of England, we rely on common trends in the differential between eligible and non-eligible people in the pilot areas and the rest of England.

In the period covered by our data, there were two major policy changes which may have affected the incentives facing young people in their decision to continue in education. These were the introduction of the minimum wage and changes in tax credits. If these policies affected the proportions of young people entering post-16 education in the pilot areas differently to the rest of England, our estimates may be biased.

The introduction of the minimum wage in the UK may have changed the incentives facing young people in their decision to stay in education. The minimum wage, if above the equilibrium wage, may have reduced the probability of a school-leaver finding employment but increase wages conditional on employment, and thus negatively impact on school enrolment. This would bias our results if the minimum wage had more bite in the pilot than in the rest of the country. However Table 3.9 shows this not to be the case so the introduction of the minimum wage is unlikely to have driven the estimate of the EMA roll-out down.

Additionally there were significant changes in the tax credit system. Tax credits are supplemental income received by lower income working families. Since young people aged 16 or 17 are classified as children if they attend full-time education, the incentives to stay in education have increased though time. Since the Pilot areas were poorer to begin with, the changes tax credit system, may have had a disproportionate effect on them. In 1999 the Working Family Tax Credit, replaced Family Credit. The new system was more generous to existing recipients and broadened eligibility to less-well off families further up the income distribution as there was a reduction in the rate at which the credit was reduced as family income grew. There were later changes but these

were more modest in comparison and would have had less effect on the incentives to attend post-16 education<sup>56</sup>.

If there were a greater proportion of households in pilot areas than in the rest of England who benefited from the changes in the tax credit system, this may be biasing our estimate of the Pilot EMA effect upwards as we are confounding the effect of EMA with that of the introduction of Working Family Tax Credit. For WFTC eligible families, a child leaving education reduces household income with the loss of the EMA and the tax credit assuming the child's labour income would not have made up the lost amount.

These reasons could affect the validity of the common trends assumption between pilot and rest of the country, needed to estimate difference in difference estimates. We thus re-estimate the model with a reduced control group composed of the less wealthy LEAs only. Our measure of the relative poverty of an LEA is the proportion of students in the area who receive free school meals (FSM). FSMs are provided to the children of parents who receive Income Support, income-based Jobseeker's Allowance, Working Tax Credit, etc. Only 10% of students living in pilot LEAs, live in areas where less than 10% of students are receiving Free School Meals. Whereas just under half of students in non-Pilot LEAs live in areas where less than 10% of students receive Free School Meals. The

<sup>56</sup> Children's Tax Credit was introduced in 2001. In 2002 Child Tax Credit replaced various income related payments for children including WFTC and Children's Tax Credit. .

LEAs involved in the pilot scheme were thus poorer and may have been affecting differently by the other policies introduced concomitantly to EMA pilot and roll-out.

Table 3.10 shows the estimates of the effect of the pilot scheme and the national extension in LEAs where more than 10% of students receive Free School Meals. Here we see some evidence of the effect of the national extension on eligible youth. Estimates of the effect of the pilot programme are similar as before but much more precisely estimated.

Another approach to comparing the pilot areas with similar LEAs would be to examine the outcomes in the LEAs used by Dearden et al (2009) as control areas. Dearden et al (2009) use nine of the pilot areas which started the scheme in 1999 and nine non-pilot areas. These nine control areas were selected on the basis of socio-economic characteristics, etc. None of the nine non-pilot areas were in the 41 LEAs which became pilot areas in 2000. Unfortunately because of the small number of LEAs used in their analysis it is not possible to use the YCS data to estimate the Average Treatment Effect on the Treated. However Figure 3.5 shows the average proportion of young people remaining in education beyond the age of 16 in the pilot areas and non-pilot areas used by Dearden et al (2009). Due to changes in the way in which official statistics are collected it is not possible to examine the series before 1998. However in 1999 and 2000, the rate of staying on in the pilot areas is higher than the non-pilot areas. This is consistent with the results of Dearden et al (2009). However in the early 2000's both areas experience similar trends in young people staying in education. When the EMA scheme is introduced nationally in 2004/2005, the non-pilot areas experience a sharp rise in the proportion of

young people staying on in education. The pilot areas also experience a rise but not as sharp as that for the non-pilot areas.

### **3.7 Conclusion**

In this chapter, we compare the effects of the pilot implementation of a policy and of its national implementation, thus we assess the external validity of pilot estimates. This is an important issue as piloting programmes before their national implementations has become more common in public policy. However, while estimates from pilot programmes are believed to have high internal validity due to the experimental or quasi-experimental design, little work has been done on their external validity. There are several reasons to believe that estimates from pilots and national implementations may differ substantially, in which case the external validity of pilot estimates would be small and the information that we would gain from running pilots is limited.

Our estimates suggest that the pilot scheme did have an effect on the targeted group. Although we find some evidence of the national scheme having an effect, this evidence is not as strong or robust to sensitivity checks than the evidence supporting the pilot scheme. Perhaps this is because the national implementation is less generous in real terms and targets a poorer group of young people who are less likely to stay in education full-time. In order for pilot programmes to be informative about the effects on the general population, policy makers should set thresholds and payment levels of pilots schemes at amounts which could be realistically maintained fiscally were the programme expanded to a much larger area.

Most of the significant effects found in this chapter relate to young females rather than males. This is consistent with findings of other policies interventions in the education sector where significant effects were only found for females (e.g. Angrist, Lang



and Oreopoulos (2009). It is currently an open research question as to why females respond better to interventions.

Overall, our results are also consistent with short-run credit constraints playing a role in the decisions of young people and that targeting these people has an effect on their educational outcomes.

## References for Chapter Three

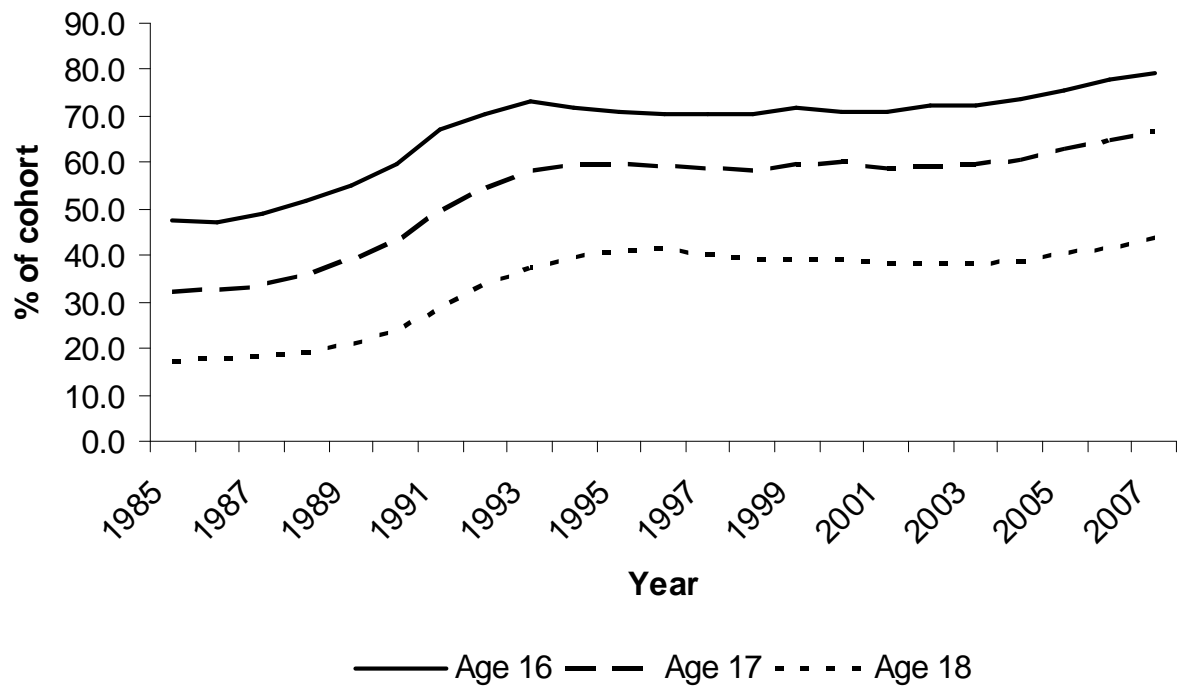
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## Figures for Chapter Three

Figure 3.1 Participation in Post-Compulsory Education by Age: England 1985-2007

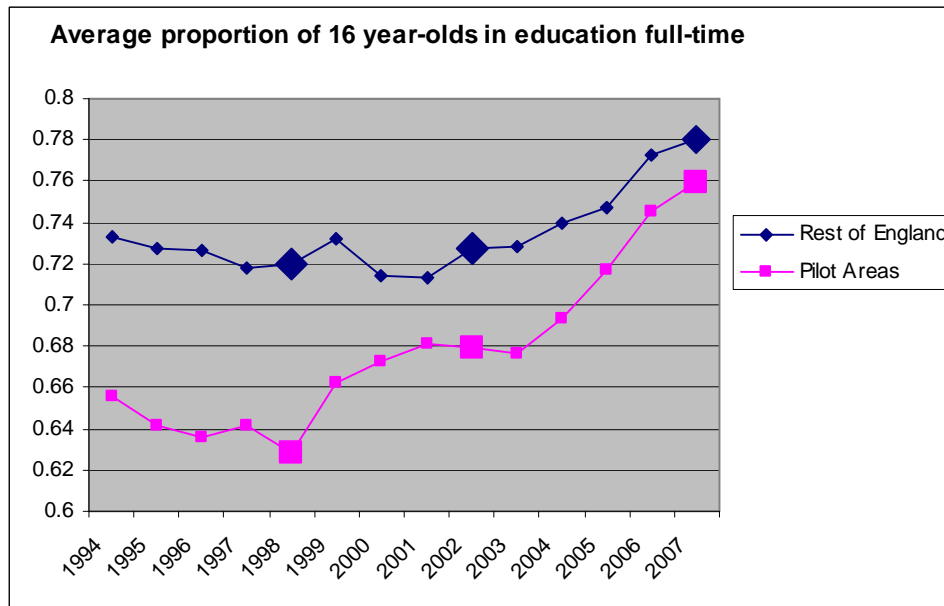


Source:

DCSF:

<http://www.dcsf.gov.uk/rsgateway/DB/SFR/s000792/index.shtml>

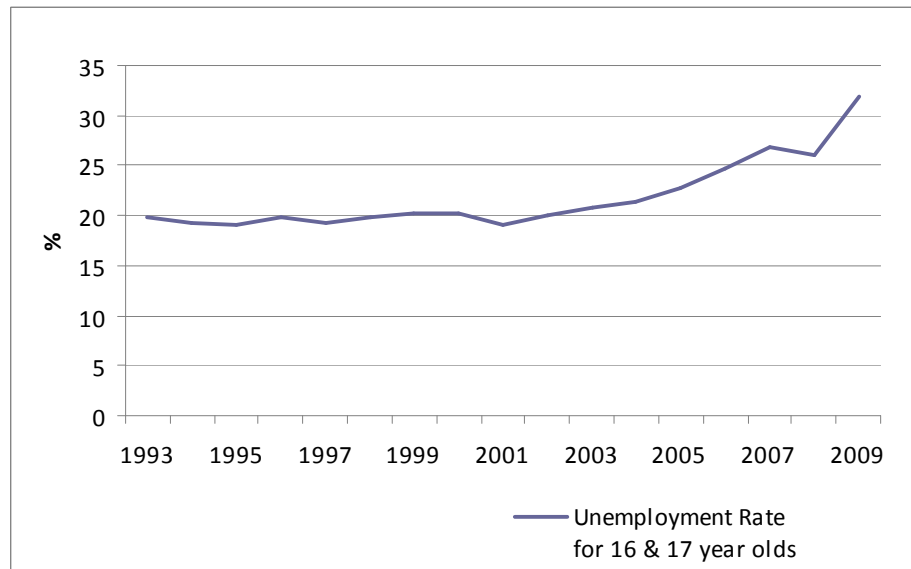
**Figure 3.2 Participation in Post-Compulsory Education**



Source: DfES: <http://www.dfes.gov.uk/rsgateway/DB/SFR/s000734/index.shtml>

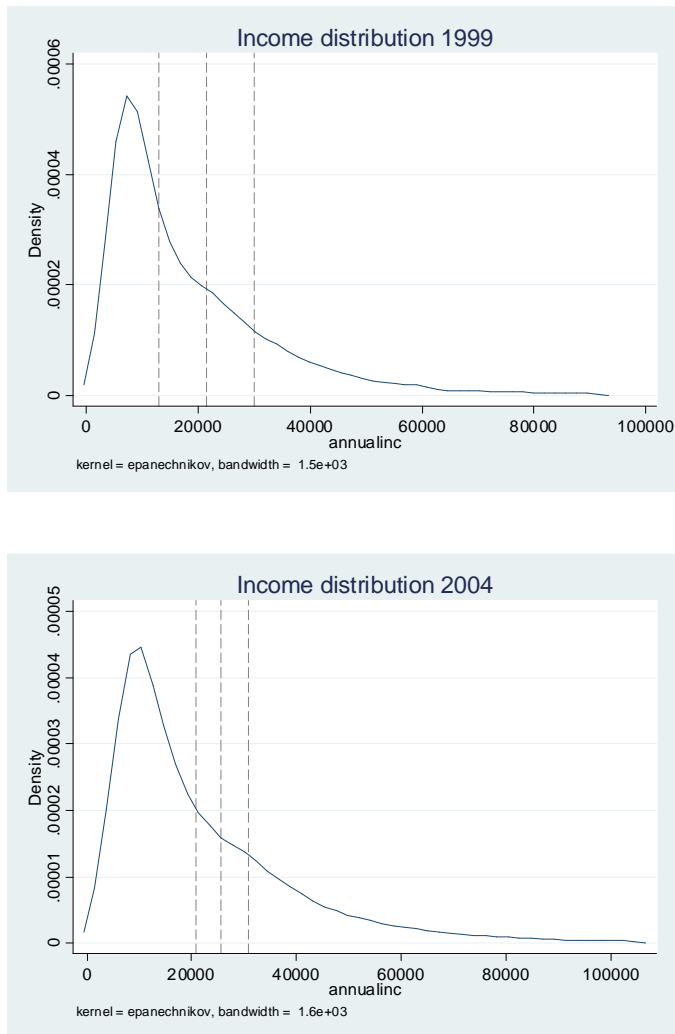
Excludes London (Inner and Outer) LEAs as those areas could not be disaggregated.

**Figure 3.3 Rate of Unemployment in UK for 16-17 year olds**



**Source: Office for National Statistics (UK)**

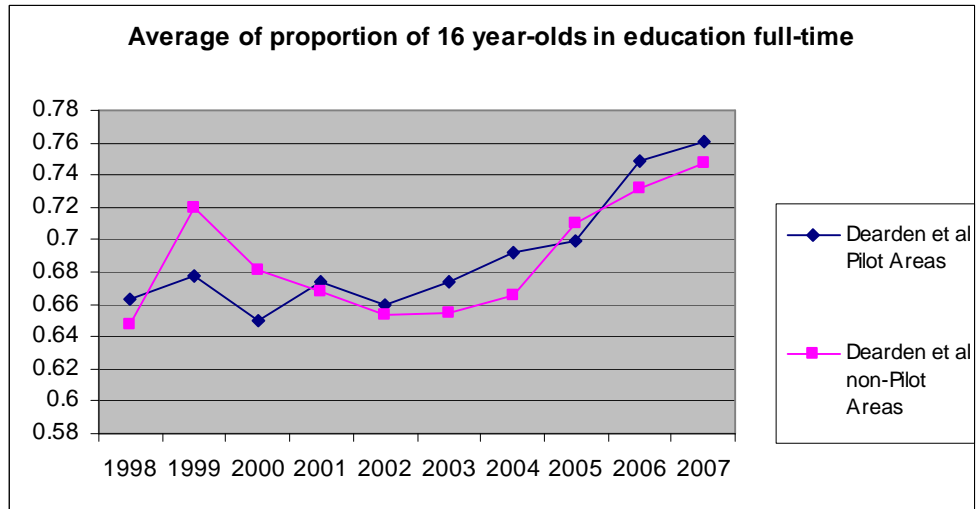
**Figure 3.4 Distribution of Household Income and EMA Eligibility Criteria**



Source: FRS 1999 and 2004



**Figure 3.5 Participation in Post-Compulsory Education**



Source: DfES: <http://www.dfes.gov.uk/rsgateway/DB/SFR/s000734/index.shtml>

Excludes London (Inner and Outer) LEAs as those areas could not be disaggregated.

## Tables for Chapter Three

**Table 3.1 Trends in Post-Compulsory Education Pre-Treatment. LEA Level Data**

	1994-1999	2000-2004
Treatment*trend	0.285 (0.358)	0.137 (0.301)
Treatment	-9.737 (2.229)	3.561 (1.994)
Trend	-0.710 (0.294)	0.448 (0.263)
Constant	75.670 (1.813)	64.328 (2.384)
R <sup>2</sup>	0.256	0.119
N	458	402

Source: DfES: <http://www.dfes.gov.uk/rsgateway/DB/SFR/s000734/index.shtml>

Excludes London (Inner and Outer) LEAs as those areas could not be disaggregated. Change in the geographies of LEAs means that the panel is unbalanced. Conclusions regarding the non-significance of the treatment specific trend estimate are not sensitive to excluding the LEAs with changing geography, or restricting the data to years with consistent geography.

Estimates are based on OLS regressions, weighted by population size of 16 year olds in 2006. Standard errors are corrected for clustering at the LEA level

**Table 3.2 Descriptive Statistics of the Family Backgrounds and Characteristics of YCS Respondents**

	Original Pilots			Rest of England		
	YCS 9	YCS 11	YCS 13	YCS 9	YCS 11	YCS 13
Father present:	71.86	69.54	67.96	79.81	79.03	73.87
Mother present:	88.28	90.65	92.51	91.71	93.18	94.2
Father working:	67.17	72.45	81.36	83.68	87.57	88.75
Mother working:	64.12	58.01	66.89	78.26	75.86	76.09
Father's Highest Qualification:						
Less than A level	72.01	66.14	69.16	64.68	53.93	60.33
A level	8.63	14.86	12.67	11.57	19.01	14.06
Degree	19.36	19.00	18.17	23.74	27.05	25.61
Mother's Highest Qualification:						

Less than A level	76.38	69.07	69.36	69.5	57.06	62.15
A level	8.83	16.41	14.93	13.62	21.55	15.73
Degree	14.79	14.52	15.71	16.88	21.39	22.12
Racial Background:						
White	78.47	79.53	78.81	92.09	92.45	92.68
Black	4.37	5.57	6.34	1.35	1.09	1.69
Asian	15.61	12.03	11.44	5.76	5.07	3.55
Mixed	1.54	2.86	3.41	0.8	1.39	2.08
Less than 5 A*-C grades at GCSE	56.88	57.08	42.68	47.06	41.7	35.25
Still in education Spring after final compulsory year	69.19	70.43	71.04	73.29	74.85	74.99
N	1692	2123	1815	5156	5611	2812

**Table 3.3**      **Estimates of Eligibility for EMA**

	YCS 9	YCS 11	YCS 13
Total number of observations in English LEAs	13762	14894	7523
MINUS:			
Missing information to infer paternal earnings for working fathers	3854	3606	1314
Missing information to infer maternal earnings for working mothers	828	534	453
Father self-employed**	1710	2126	595
Missing outcome variable or control variables or living in Transport EMA*	30	263	255
Total number of observations for which eligibility/non-eligibility inferred	6848	7734	4627

\* these were LEAs where EMA was paid towards transport costs of students. As this is quite a different scheme, these areas were ignored.

\*\*LFS only used to infer income for employees only

**Table 3.4**      **Estimates of Eligibility for EMA**

	ALL			Original Pilots			Rest of England		
	YCS 9	YCS 11	YCS 13	YCS 9	YCS 11	YCS 13	YCS 9	YCS 11	YCS 13
Predicted eligible for EMA	73.8	68.03	53.27	83.31	74.32	54.77	62.91	55.99	43.49
In receipt of EMA	0.00	20.47	44.66	0.00	50.12	49.54	0.00	5.77	38.80
“Not Eligible” for EMA but in receipt of EMA.	0.00	1.4	7.01	0.00	3.72	5.95	0.00	0.50	7.8
“Eligible” for EMA, attending school but not in receipt	0.00	30.47	0.94	0.00	0.25	0.93	0.00	35.29	0.94
N	6848	7734	4627	1692	2123	1815	5156	5611	2812

All expressed as a percentage of total number of student in corresponding year and treatment/control group

**Table 3.5      Background of YCS respondents by EMA eligibility**

EMA eligible							Not EMA eligible						Inflation adjusted
Original Pilots				Rest of England			Original Pilots			Rest of England			All
	YCS 9	YCS 11	YCS 13	YCS 9	YCS 11	YCS 13	YCS 9	YCS 11	YCS 13	YCS 9	YCS 11	YCS 13	YCS 13
Father present:	66.23	59.80	44.00	67.91	62.6	39.34	100.00	100	99.71	100	100	99.93	54.34
Mother present:	86.26	88.54	87.94	87.43	88.81	88.57	98.41	97.28	98.57	98.98	98.7	98.45	90.97
Father working:	59.15	60.36	50.94	72.36	75.08	52.59	100	100	100	100	100	100	71.45
Mother working:	56.77	47.3	43.06	66.48	62.9	50.29	99.09	91.42	96.1	97.41	91.85	94.16	60.22
Father's Highest Qualification:													
Less than A level	90.12	87.98	86.52	87.03	82.35	78.98	13.55	23.53	59.26	37.57	28.29	54.99	88.88
A level	3.86	6.54	8.53	6.28	10.55	13.83	24.03	30.9	15.03	17.99	26.63	14.13	7.24
Degree	6.02	5.48	4.95	6.68	7.10	7.20	62.43	45.57	25.71	44.44	45.09	30.88	3.89
Mother's Highest Qualification:													
Less than A level	87.84	84.22	77.01	84.5	77.77	68.38	32.57	29.66	60.62	48.69	34.09	57.99	78.38

A level	6.11	8.67	13.63	8.88	12.95	14.29	19.2	36.75	16.42	20.18	31.13	16.70	10.67
Degree	6.05	7.11	9.35	6.61	9.28	17.33	48.23	33.59	22.97	31.12	34.78	25.32	10.95
Racial Background:													
White	75.67	75.9	69.49	89.31	90.16	88.81	92.25	90.97	90.87	96.74	95.28	95.58	85.28
Black	4.76	6.56	8.6	1.88	1.5	2.52	2.45	2.39	3.41	0.46	0.58	1.08	3.87
Asian	17.89	14.96	16.85	7.85	7.1	5.76	4.41	2.84	4.45	2.27	2.55	1.89	7.73
Mixed	1.68	2.59	5.07	0.95	1.24	2.9	0.88	3.8	1.27	0.53	1.59	1.46	3.12
Less than 5 A*-C													
grades at GCSE	63.4	64.83	52.1	58.2	53.47	42.81	24.27	32.31	30.21	28.15	26.32	29.55	44.91
Still in education	65.81	67.55	66.97	66.37	68.97	72.4	86.12	81.47	76.43	85.02	82.96	76.94	70.22
N	1436	1631	1324	3405	3363	1494	256	492	491	1751	2248	1318	3416



**Table 3.6 Linear Probability Model– Effect of EMA Pilot Scheme on Post-Compulsory Full-Time Education of Eligible Youth**

	A- Predicted eligibility			B- Recoding all recipients to eligible			C- Recoding ineligible recipients to missing		
<b>PILOT estimates</b>	Male & female	Male only	Female only	Male & female	Male only	Female only	Male & female	Male only	Female only
Average Treatment Effect on Treated	0.022	-0.019	0.065	0.103	0.04	0.163	0.055	0.002	0.107
Standard Error	0.038	0.064	0.046	0.041*	0.067	0.045**	0.04	0.067	0.045*
Sample Size	14612	6576	8036	15308	6857	8451	14503	6538	7965

Standard errors are adjusted for clustering at the LEA level.

+, \* and \*\* indicate significant at the 10%, 5% and 1% level respectively.

**Table 3.7 Linear Probability Model– Effect of EMA Roll-Out on Post-Compulsory Full-Time Education of Eligible Youth.**

	A- Predicted eligibility			B- Recoding all recipients to eligible			C- Recoding ineligible recipients to missing		
<b>Roll-out estimates</b>	Male & female	Male only	Female only	Male & female	Male only	Female only	Male & female	Male only	Female only
Average Treatment Effect on Treated	0.034	0.048	0.018	0.083	0.062	0.100	0.046	0.033	0.055
Standard Error	0.041	0.066	0.055	0.044 <sup>+</sup>	0.070	0.048*	0.045	0.074	0.055
Sample Size	12361	5633	6728	13894	6329	7565	11927	5433	6494

Standard errors are adjusted for clustering at the LEA level.

<sup>+</sup>, \* and \*\* indicate significant at the 10%, 5% and 1% level respectively.

**Table 3.8 Linear Probability Model – Effect of EMA Pilot and Roll Out on Post-Compulsory Full Time Education by GCSE Achievement**

	A- Predicted eligibility				B- Recoding all recipients to eligible			
	Students <5 A*- C GCSE grades		Students +5 A*- C GCSE grades		Students <5 A*- C GCSE grades		Students +5 A*-C GCSE grades	
	Male only	Female only	Male only	Female only	Male only	Female only	Male only	Female only
<b>PILOT</b>								
Average Treatment Effect on Treated	-0.2	0.076	0.061	0.08	-0.128	0.273	0.113	0.119
Standard Error	0.153	0.148	0.04	0.038*	0.158	0.148+	0.043**	0.039**
Sample Size	2760	2854	4097	5597	2760	2854	4097	5597
<b>ROLL OUT</b>								
Average Treatment Effect on Treated	0.052	-0.072	0.019	0.063	-0.034	0.165	0.076	0.057
Standard Error	0.127	0.157	0.05	0.049	0.143	0.162	0.052	0.046
Sample Size	2523	2560	3806	5005	2523	2560	3806	5005

Standard errors are adjusted for clustering at the LEA level. Standard errors are robust to heteroskedasticity.

+, \* and \*\* indicate significant at the 10%, 5% and 1% level respectively.

**Table 3.9 Rates of Minimum Wage & Average Youth Wages**

	16-17 Year Olds Minimum Wage	Average Youth Wages Non- Pilots Areas	Standard Deviation Youth Wages Non-Pilots Areas	Average Youth Wages Pilots Areas	Standard Deviation Youth Wages Pilots Areas
1998-1999		2.99	1.25	2.81	1.56
1999-2000	-				
2000-2001	-				
2001-2002	-	3.75	1.64	3.50	1.89
2002-2003	-				
2003-2004	-				
2004-2005	£3.00				
2005-2006	£3.00				
2006-2007	£3.30	4.80	1.03	4.77	1.29
2007-2008	£3.40				
2008-2009	£3.53				
2009-2010	£3.57				

Source: <http://www.lowpay.gov.uk/> and YCS 9, YCS 11 and YCS 13.

**Table 3.10 Linear Probability Model – Effect of EMA Pilot Scheme on Post-Compulsory Full-Time Education of Eligible Youth in Areas Where More Than 10% of Students Receive Free School Meals.**

	A- Predicted eligibility				B- Recoding all recipients to eligible			
	Students <5 A*-C		Students +5 A*-C		Students <5 A*-C		Students +5 A*-C	
	GCSE grades		GCSE grades		GCSE grades		GCSE grades	
	Males	Females	Males	Females	Males	Females	Males	Females
<b>PILOT</b>								
Average Treatment Effect on Treated	-0.257	0.05	0.077	0.111	-0.192	0.236	0.127	0.147
Standard Error	0.16	0.158	0.043+	0.041**	0.165	0.159	0.046**	0.042**
Sample Size	2119	2166	2816	3902	2231	2319	2969	4134
<b>ROLL OUT</b>								
Average Treatment Effect on Treated	0.083	-0.164	0.028	0.118	-0.017	0.067	0.106	0.103
Standard Error	0.137	0.169	0.052	0.055*	0.155	0.176	0.056+	0.052*
Sample Size	1704	1727	2315	3040	1977	2006	2643	3486

Standard errors are adjusted for clustering at the LEA level. Standard errors are robust to heteroskedasticity.

+, \* and \*\* indicate significant at the 10%, 5% and 1% level respectively.

## Appendix for Chapter Three

**Table 3A1      Linear Probability Model– Effect of EMA Pilot Scheme on Post-Compulsory Full-Time Education of Eligible Youth**

	A- Predicted eligibility			B- Recoding all recipients to eligible			C- Recoding ineligible recipients to missing		
	All	Males	Females	All	Males	Females	All	Males	Females
Eligible	-0.082	-0.097	-0.065	-0.084	-0.099	-0.068	-0.081	-0.097	-0.064
	0.012	0.016	0.016	0.012	0.016	0.016	0.012	0.016	0.016
After Policy									
Change	-0.026	-0.042	-0.010	-0.028	-0.044	-0.011	-0.028	-0.044	-0.012
	0.010	0.019	0.013	0.010	0.019	0.013	0.010	0.019	0.013
Eligible*After									
Policy Change	0.033	0.065	0.001	0.045	0.074	0.015	0.035	0.066	0.003
	0.016	0.025	0.020	0.016	0.025	0.020	0.016	0.025	0.020
LEA Where									
Policy									
Changed	0.010	-0.019	0.039	0.011	-0.018	0.039	0.010	-0.019	0.039
	0.019	0.032	0.024	0.019	0.032	0.024	0.019	0.032	0.024

Eligible* LEA									
Where Policy									
Changed	0.002	0.057	-0.052	0.002	0.056	-0.053	0.003	0.057	-0.051
	0.028	0.042	0.033	0.028	0.042	0.033	0.028	0.042	0.033
After Policy									
Change*LEA									
Where Policy									
Change	-0.011	0.002	-0.025	-0.044	-0.019	-0.068	-0.043	-0.019	-0.067
	0.033	0.054	0.040	0.037	0.059	0.041	0.037	0.059	0.041
Eligible*After									
Policy									
Change*LEA									
Where Policy									
Change	0.022	-0.020	0.066	0.103	0.040	0.163	0.055	0.002	0.107
	0.038	0.064	0.046	0.041	0.067	0.045	0.040	0.067	0.045
Less than 5									
A*-C GCSE									
grades	-0.358	-0.382	-0.334	-0.350	-0.376	-0.323	-0.360	-0.383	-0.337

	0.009	0.013	0.011	0.009	0.013	0.010	0.009	0.013	0.011
Male	-0.036	0.000	0.000	-0.040	0.000	0.000	-0.035	0.000	0.000
	0.008	0.000	0.000	0.007	0.000	0.000	0.008	0.000	0.000
Sample Size	14612	6576	8036	15308	6857	8451	14503	6538	7965



**Table 3A2      Linear Probability Model – Effect of EMA Pilot and Roll Out on Post-Compulsory Full Time Education by GCSE Achievement**

	A- Predicted eligibility			B- Recoding all recipients to eligible			C- Recoding ineligible recipients to missing		
	All	Males	Females	All	Males	Females	All	Males	Females
Eligible	-0.028	-0.004	-0.052	0.060	0.063	0.054	0.012	0.026	-0.005
	0.024	0.046	0.027	0.026	0.046	0.022	0.027	0.048	0.024
After Policy									
Change	-0.061	-0.022	-0.101	-0.068	-0.056	-0.084	-0.070	-0.059	-0.083
	0.032	0.052	0.039	0.039	0.060	0.041	0.039	0.060	0.041
Eligible*After									
Policy Change	0.012	-0.022	0.045	0.017	0.030	0.009	0.019	0.013	0.027
	0.036	0.060	0.047	0.039	0.064	0.041	0.041	0.067	0.047
LEA Where									
Policy									
Changed	-0.001	0.011	-0.010	0.034	0.036	0.031	0.033	0.035	0.030
	0.021	0.038	0.026	0.025	0.043	0.027	0.025	0.042	0.027
Eligible* LEA									
Where Policy	-0.024	-0.033	-0.016	-0.105	-0.096	-0.112	-0.059	-0.058	-0.058

Changed									
	0.027	0.050	0.030	0.029	0.050	0.026	0.029	0.052	0.028
After Policy									
Change*LEA									
Where Policy									
Change	0.013	-0.020	0.044	-0.001	-0.007	0.007	0.001	-0.005	0.008
	0.035	0.056	0.044	0.042	0.064	0.046	0.042	0.064	0.046
Eligible*After									
Policy									
Change*LEA									
Where Policy									
Change	0.034	0.048	0.018	0.083	0.062	0.100	0.046	0.033	0.055
	0.041	0.066	0.055	0.044	0.070	0.048	0.045	0.074	0.056
Less than 5									
A*-C GCSE									
grades	-0.344	-0.367	-0.320	-0.328	-0.349	-0.306	-0.354	-0.378	-0.330
	0.009	0.013	0.012	0.009	0.012	0.011	0.009	0.013	0.012
Male	-0.033	0.000	0.000	-0.036	0.000	0.000	-0.034	0.000	0.000

	0.009	0.000	0.000	0.008	0.000	0.000	0.009	0.000	0.000
Sample Size	12361	5633	6728	13894	6329	7565	11927	5433	6494

**Table 3A3 Descriptive Statistics of EMA recipients and imputed eligibility status**

	EMA recipient but imputed to be ineligible	EMA recipient but imputation could not be made	In receipt of EMA and imputed to be eligible
Father present:	99.68	76.32	44.98
Mother present:	97.92	94.41	91.14
Father working:	100	87.68	47.5
Father self-employed	0	61.24	0
Mother working:	94.44	70.61	45.09
Father's Highest Qualification:			
Less than A level	64.2	70.77	86.3
A level	17.09	15.3	8.38
Degree	18.71	13.93	5.33
Mother's Highest Qualification:			
Less than A level	68.71	72.01	76.46

A level	17.38	16.68	13.05
Degree	13.9	11.31	10.49
Racial Background:			
White	94.89	80.79	77.27
Black	1.36	6.02	5.34
Asian	2.14	10.81	13.97
Mixed	1.61	2.38	3.42
Less than 5 A*-C grades at GCSE	38.41	46.43	48
Still in education	88.02	89.87	88.46
N	434	1533	2675